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ORIGINAL ARTICLES.

I.—Further Contributions to Knowledge of the Devonian Fish-fauna of Canada.

By Arthur Smith Woodward, F.L.S., F.G.S.

(PLATE XIII.)

EARLY in the year the present writer published some notes on the Lower Devonian Fishes of Campbellton, New Brunswick, collected by Mr. Jex in the summer of 1891.[1] Since that date the remainder of the collection, comprising a very large series of specimens from the Upper Devonian of Scaumenac Bay, in the Province of Quebec, has been acquired by the British Museum; and materials are thus forthcoming for some further observations.

I. On the Body-armour of *Phlyctænaspis acadica*.

Before, however, proceeding to a consideration of the fishes of the Upper Devonian, a few additional examples of *Phlyctænaspis* from Campbellton are worthy of brief note, in reference to the body-armour. The presence of a number of plates, much resembling those of the ordinary *Coccosteus*, has already been determined by Whiteaves;[2] the median dorsal and the ventro-lateral plates being especially similar. It is also evident, from certain of the new specimens (Brit. Mus., Nos. P. 6574–75), that the body-armour in *Phlyctænaspis* is articulated with the head-shield by means of a boss on each anterior dorso-lateral plate, exactly as in *Coccosteus*. There are, nevertheless, certain small plates that cannot be placed with reference to the last-named familiar genus; and it is not unlikely that some of these—notably the symmetrical, ridged examples—will prove to occur on the otherwise unarmoured tail.

But the most striking feature in the body-armour of *Phlyctænaspis*, now shown for the first time, is the presence of a pair of fixed spines, each apposed in the greater part of its extent to a lateral plate of the trunk (Fig. 1). These spines are robust but hollow, compressed, very slightly arched, tapering at both ends, and marked with irregular longitudinal series of tubercles. They are, indeed, precisely similar in external form and appearance to those of

[1] Smith Woodward, "On the Lower Devonian Fish-Fauna of Campbellton, New Brunswick," Geol. Mag. Dec. III. Vol. VIII. pp. 1–6, Pl. I. (1892).

[2] J. F. Whiteaves, "Illustrations of the Fossil Fishes of the Devonian Rocks of Canada, Part I.," Trans. Roy. Soc. Canada, vol. vi. sect. iv. pp. 94, 95, pl. ix. (1888).

Acanthaspis;[1] and so far as can be judged from known specimens, they only differ from the last-mentioned spines in the circumstance, that the supporting plate is destitute of the extended oblique pedicle observed both in the type specimens from the Corniferous Limestone of Ohio and in the shield assigned to the same genus from Spitzbergen.[2] It thus remains to discover more associated examples of the plates and spines from Ohio, to determine whether they actually pertain to Ostracoderms, as suspected, or whether they represent part of the armour characteristic of Arthrodira; for the fixed spinous appendage is now proved to occur in both of these widely separated groups.

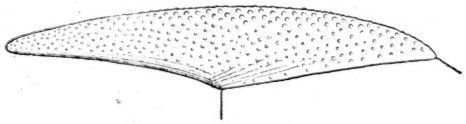


Fig. 1.—Spinous appendage of dermal armour of *Phlyctænaspis acadica*.

II. New Species from the Upper Devonian, Scaumenac Bay.

The fossils from Scaumenac Bay comprise numerous fine examples of the known species of *Bothriolepis*, *Acanthodes*, *Phaneropleuron*, *Eusthenopteron*, and *Cheirolepis*. Among them there are also remains of two additional genera, hitherto not discovered in Canada; and the best of these specimens are shown in Plate XIII. One genus is the Acanthodian *Diplacanthus*, only known previously from the Lower Old Red Sandstone of Scotland; the other is *Coccosteus*, of very wide range in the Old Red Sandstone, both as regards time and space.

Diplacanthus horridus, sp. nov. [Plate XIII. Fig. 1.]

Sp. Char.—A species of moderate size, attaining a length of not less than 0·12; the greatest depth of the trunk contained about four times in the total length of the fish. Fin-spines much elongated, with one deep longitudinal sulcus parallel to the anterior margin, and the sides either smooth or very finely striated. [Pectoral fin-spines incompletely known]: median pectorals conspicuous, close to the former; pelvic fin-spines relatively large, about equalling the anal fin-spine in size. Dorsal fin-spines very large and elongated, the length of the first much exceeding the depth of the trunk at its point of insertion, equalling the length of the space between the two dorsal fin-spines, and larger than the second fin-spine, which is directly opposed to the anal fin-spine, and equals the latter in size. Scales marked with prominent radiating furrows and ridges; those of the lateral line in the abdominal region slightly enlarged.

Specimens.—This species is determined on the evidence of two specimens, of which the type, in counterpart, is shown of three-

[1] J. S. Newberry, "Fishes of the Devonian System," Geol. Surv. Ohio, vol. ii. pt. ii. p. 36 (1875).

[2] Smith Woodward, "The Devonian Fish-Fauna of Spitzbergen," Ann. Mag. Nat. Hist. [6] vol. viii. p. 4, pl. i. (1891).

quarters the natural size in Pl. XIII. Fig. 1. The head and extremity of the tail are imperfect in both specimens, and the pectoral fins are only fragmentary; but otherwise the general form and proportions of the fish are well shown.

In the head the large dermal tesseræ on the cranial roof, and part of the narrow ring round the orbit (*orb.*), are exhibited by both specimens; and there is a pair of small elements postero-inferiorly (*s.*), each with a triangular expansion at one end, a contraction mesially, and a less expansion at the other end, probably representing the "styliform bone." Behind the orbit in the type specimen, there is also evidence of a vertically-elongated, superficially-calcified cartilage, apparently the hyomandibular. The ascending portion of the scapular arch (*x.*) is distinct behind the head; and in the type-specimen one of the median spines (*m.*) is well shown immediately within the base of the pectoral fin-spine (*pct.*). The intermediate ventral spines (*i.*) are well developed and exhibited in both specimens; and the very large pelvic fin-spines (*plv.*) also occur in both. All the median fin-spines are more or less fractured, though their proportions are satisfactorily indicated (d_1. d_2. *a.*); and in the type specimen the fin-membrane is seen to extend to the extremity of the spine in the two dorsals. All the fin-spines exhibit a single deep groove close to and parallel with their anterior border; and the lateral face behind this groove is feebly marked with longitudinal striations, especially in the anal fin of the second specimen. The caudal fin (*c.*) shows the ordinary Acanthodian characters, and there are traces of the robust, calcified hæmal arches of the axial skeleton at the base of its lower lobe. The scales (Fig. 1*a*) are very conspicuously ornamented, and the slight enlargement of the two rows of scales bordering the lateral line in the abdominal region is distinct in both specimens.

Affinities.—The species thus indicated belongs to the genus *Diplacanthus*, as defined in the British Museum Catalogue (Pt. II. p. 23), and differs notably from the most closely allied species, *D. longispinus*, in the very large size of the median fin-spines, and in their relative dimensions as noted in the diagnosis.

Coccosteus canadensis, sp. nov. [Plate XIII. Fig. 2.]

This species as yet is not satisfactorily definable, being known only by a weathered beach-pebble exhibiting an impression of the head-shield. The features shown, however, suffice to readily distinguish this shield from all described forms except the typical *Coccosteus decipiens*; and from the head-shield of the latter it evidently differs (i) in its greater length as compared with the breadth, (ii) in the narrower median occipital, and (iii) in the relatively smaller size of the central plates.

Almost the whole of the border of the shield is destroyed, but most of the sutures and the sensory canals are distinctly exhibited in impression. The median occipital plate (*m. occ.*) is considerably more than twice as broad behind as in front, and its superficial tuberculations are arranged in radiating series towards the posterior

border. The lateral occipital (*l. occ.*), marginal (*m.*), preorbital (*p. o.*), postorbital (*pt. o.*), and pineal (*p.*) plates are imperfect and do not require special note; while the central plates (*c.*) form a relatively small and not quite symmetrical pair. The sensory canals of each side are distinctly united in the usual manner by a transverse commissure across the central plates; and it may be added that the white mark on the lateral occipital of the right side in the drawing is an indication merely of a fracture.

Coccosteus is already well known to occur in the typical Upper Old Red Sandstone; and it is interesting to note that in Russia, as in Canada, the genus is found in association with species of *Bothriolepis*.

III. On the Supposed Jaws of *Bothriolepis*.

In the new collection the British Museum acquires many fine examples of *Bothriolepis*, some displaying features not hitherto observed; but the most striking specimens are two exhibiting the supposed jaws.

It has long been known that a pair of loose, narrow plates occurs on the inferior face of the head in the Asterolepidæ. These plates were interpreted by Pander,[1] as the two rami of the lower jaw; and they are placed by Traquair[2] in juxtaposition with the anterior border of the ventral body-shield as "mental plates," of uncertain, though possibly mandibular function. By Whiteaves,[3] however, the pair of elements in *Bothriolepis* is represented as immediately adjoining the front margin of the dorsal shield, while the space for the mouth occurs behind. It is the latter interpretation that now proves to be correct, and a diagrammatic sketch of the parts in the critical specimen (No. P. 6761) is given in the accompanying Woodcut, Fig. 2.

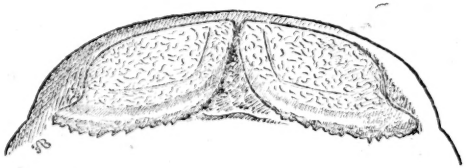


Fig. 2.—Supposed jaw-plates of *Bothriolepis canadensis*.

This drawing, it will be observed, agrees well with that of the corresponding parts already given by Whiteaves (*loc. cit.* pl. vii.) merely adding some important details. In all the specimens of *Bothriolepis canadensis* in which the present writer has been able to examine the plates in question, they occur in close apposition to the anterior margin of the head-shield as here shown; and it is noteworthy that there are traces of an excavation of the short externo-lateral margin of each of these plates, as if providing space

[1] C. H. Pander, "Die Placodermen des Devonischen Systems," p. 49 (1857).
[2] R. H. Traquair, "On the Structure and Classification of the Asterolepidæ," Ann. Mag. Nat. Hist. [6] vol. ii. p. 488, pl. xvii. fig. 2 (1888).
[3] Trans. Roy. Soc. Canada, vol. iv. sect. iv. pl. vii. (1886).

for a pair of nasal openings at the antero-lateral angles of the snout. Mesially, the plates taper somewhat to the symphysis, being in this respect unlike those of *Pterichthys*; and the free posterior border is convexly arched. The greater part of the outer face of each plate is feebly rugose and marked by a sharply bent sensory canal; but there is a conspicuous smooth band immediately adjoining the posterior border, and this is further remarkable for exhibiting an irregular series of minute sharp denticles different in aspect from the bosses and points of the ordinary surface ornament. It appears, indeed, as if the pair of plates in question formed the anterior margin of the mouth, covered with an overlapping lip of soft tissue and provided with a minute denticulation. It still remains to be determined, however, in what manner a jaw of this form can have worked; and the opposing hard parts, if any, have yet to be discovered.

It may be added that internal to these jaw-plates in one specimen (P. 6762), there occurs the very thin lamina of smooth bone already noted by Whiteaves (*loc. cit.* 1886, p. 103); and its straight hinder margin is shown across the space at the symphysis in the drawing, Fig. 2. This is evidently an internal bone, but it is difficult even to hazard a suggestion as to its homologies and function; and the interpretation of the element must be deferred until the discovery of still more satisfactory specimens.

EXPLANATION OF PLATE XIII.

Upper Devonian Fishes from Scaumenac Bay, Province of Quebec, Canada.

Fig. 1. *Diplacanthus horridus*, sp. nov.; lateral aspect of fish, three-quarters natural size. *a.* anal fin-spines; *c.* caudal fin; d_1, d_2. first and second dorsal fin-spines; *i.* intermediate ventral spines; *m.* median pectoral spine; *orb.* orbit; *pct.* pectoral fin-spine; *plv.* pelvic fin-spine; *s.* styliform bones; *x.* pectoral arch. [Brit. Mus., No. P. 6756.]

Fig. 1*a*. Scales of ditto, much enlarged.

Fig. 2. *Coccosteus canadensis*, sp. nov.; impression of external aspect of head-shield, two-thirds natural size. *c.* central plates; *l.occ.* lateral occipital; *m.* marginal; *m.occ.* median occipital; *p.* pineal; *p.o.* pre-orbital; *pt.o.* post-orbital. [Brit. Mus., No. P. 6755.]

II.—On Porphyritic Quartz in Basic Igneous Rocks.

By Alfred Harker, M.A., F.G.S.

[Read before the British Association, Section C, Edinburgh Meeting.]

ALTHOUGH the old-fashioned ideas as to the association of different minerals held by Breithaupt and others have been found to require much modification, there are still certain rules which hold with a high degree of generality. They are, indeed, merely consequences of the principle that the most important of the factors which determine the mineralogical constitution of an igneous rock, is the chemical composition of the magma from which it is formed. Roughly speaking, we may say that original free silica and acid silicates occur characteristically in acid rocks, more basic silicates in basic rocks. The striking exceptions are few and of comparatively rare occurrence. Thus the iron-olivine fayalite,

the most basic of all rock-forming silicates, has been shown to exist in certain highly acid lavas, but it is known from two or three localities only. The case we have chosen for discussion is the exact opposite of this, *viz.* the occasional presence of free silica in rocks of thoroughly basic composition.

Olivine-basalts containing porphyritic quartz have been described by the American petrologists from California, Arizona, New Mexico, and Colorado, and similar cases have been noticed by others in places as widely separated as Arran, Madagascar, and the Korea. The typical manner of occurrence of the quartz is in small, more or less rounded grains, coated with a thin layer or shell of a greenish colour. This shell is ascribed to corrosion of the quartz-grain by the molten magma in which it was enveloped; it consists for the most part of minute crystals or granules of augite, sometimes of hornblende probably pseudomorphous after augite. Except for this shell, the quartz-grains closely resemble the corroded porphyritic quartz of many quartz-porphyries.

There is, however, another group of basic and sub-basic rocks which enclose porphyritic quartz much more commonly than the basalts do, *viz.* the peculiar group of the lamprophyres (minettes, kersantites, etc.). The little grains here have the same character as in the preceding case, and are surrounded by a similar augite-shell. They are known in the lamprophyre dykes of the Harz, Spessart, Dresden, and other places, and are beautifully exhibited in the North of England, *e.g.* in the dykes which cut the Lower Palæozoic inliers of Edenside and Teesdale.

The explanation adopted by most writers is that the enclosed grains of quartz represent fragments mechanically caught up by the magma in its passage through solid rocks during the process of intrusion. Such foreign fragments are undoubtedly met with in these as in other intrusive rocks, but to suppose that more than a small proportion of the recorded cases is to be explained in this way raises obvious difficulties. On this hypothesis we should expect intrusive rocks other than lamprophyres in the same districts to carry as frequently similar fragments; we should look to find in the neighbourhood some rock from which the grains of quartz might be derived; and we should expect the grains to occur with a local distribution in the dykes which enclose them. The theory fails in these respects, and a closer examination of the grains themselves proves conclusively that they are original constituents of the rocks in which they are found. In the first place they never have the shape of fragments. Whenever the corrosion has not entirely obliterated the original form, the outlines of the hexagonal pyramid are clearly discernible. Nextly, the grains are never composite, but always optically uniform throughout their extent. Thirdly, they contain no inclusions except rare crystals of zircon, apatite, or some other mineral of early separation, and glass-cavities, presenting thus a marked contrast to the quartz of granite or gneiss and to the great bulk of the grains in sandstones and grits. To these points of evidence, collectively very strong, others might be added. Thus, in

the lamprophyres of the North of England, and apparently of some other districts, the quartz-grains are accompanied by various acid felspars. These never have the form of fragments, but of perfect crystals rounded by corrosion, and they are to be matched, not in any rocks broken through by the dykes, but in more acid rocks contemporaneous and cognate with the lamprophyres.

Diller and Iddings, clearly recognizing in the case of the American basalts the primary nature of the quartz, have supposed that this mineral separated out at an early stage from a magma having the composition of the rock in which the grains occur. Petrologists in general will, I think, be slow to admit the possibility of this. The last-named geologist[1] has pointed out that crystallization in an igneous magma may be modified by many conditions—the presence of water, variations of temperature and pressure, etc.—the effects of which we cannot always foresee; but it may fairly be objected that, if all these factors were of much importance, we should never find two specimens of igneous rocks alike. The *exceptional* nature of the phenomenon to be explained seems to preclude such general considerations.

A quite different and perhaps more plausible explanation of the presence of quartz-grains in basic rocks is suggested by an hypothesis which has recently attained some prominence in geological speculation. I refer to the conception of a large body of molten rock-material becoming separated by gravity into strata of different densities, the upper and lighter layers being the more acid, the lower and heavier more basic in composition. Postulating such a divided magma, we obtain a clue to the cognate origin of acid and basic rocks in many areas of igneous intrusion. There is another consideration essential to our argument. It seems to be established that crystals of quartz and felspar are not only denser than the magma which gives birth to them, but also denser than magmas of much more basic composition. If, then, we suppose quartz to crystallize out in the upper acid portion of a stratified magma-basin at an early stage, when the magma is still quite fluid, we see that the crystals must sink into the lower basic layers of the heterogeneous mass.

Our theory is, then, that the quartz was crystallized out, not in the basic rock in which it now occurs, but in a magma of acid composition which once overlay the basic; so that the quartz-grains scattered through our lamprophyre dykes had precisely the same origin as the quartz-crystals in the accompanying dykes of quartz-porphyry. The grains indeed differ from the crystals only in their more advanced stage of corrosion, due to the more basic nature of the enveloping magma. This process of corrosion, however, need not have taken place in the original magma-basin, and it is more probably assigned to a later time, when the great pressure was relieved, and the rocks were injected into their present position.

These ideas, suggested by a study of our north-country rocks, will probably be found to apply to the lamprophyres of other districts,

[1] J. P. Iddings, Amer. Journ. Sci. (3) vol. xxxvi. p. 208 (1888).

and may throw light also on the problem of the origin of quartz-bearing basalts. It is noteworthy that in the districts where these various abnormal quartz-bearing types occur, the association of igneous rocks of very different chemical compositions and the order of their succession lead us, on quite independent grounds, to the hypothesis of the stratified magma-basin.

III.—Note on the Lithophyses in Obsidian of the Rocche Rosse, Lipari.[1]

By H. J. Johnston-Lavis, M.D., M.R.C.S., F.G.S.

It is a pretty well recognized fact that Obsidian is a *hydrous* silicate of indefinite composition and your fusion experiments confirm that fact. Now molten obsidian may give off its H_2O in consequence of two processes—by simple vesiculation, or by the individualization from the glass of a definite *anhydrous* silicate. More commonly these two processes have gone on together, but modified by the vicissitudes of temperature that the molten aquiferous glass is exposed to during its eruption.

A free surface is alike favourable to the separation of a gas from solution, and to the starting of crystallization. Crystallization may start from a vesicle (as I could show you at this present moment in a specimen of ice) or vesicles will form on the surface of crystals.

A spherulite, however, usually starts from some solid particle, as a microlith or a grain of dust enclosed in the glass, and proceeds to grow *outwards* in a radiating manner, but as it grows it liberates some vapour by the conversion of the hydrous glass to an anhydrous felspar; and likewise the spherulite acts, when the pressure is diminishing, as a catalytic or free surface, and simple vesiculation takes place.

The crystallization vapour will collect between the outer ends of the fibres, press them apart, and be joined by its ally the vesiculation vapour. The result is that they form a spherical vapour shell around the spherulite. The highly viscous glass (as proved by faulting in it) draws with it the outer ends of the radiating needles forming the greater part of the spherulite, with the exception of the most resisting part or parts on one or more sides which carries with it the central remnants of the spherulitic concretion as shewn in your figure 3. The spherulite may have broken up into a number of cones scattered over the interior of the vesicle-walls where they are attached by their bases. At the same time the sides of the cones have shrunk by the crystallization of the remaining glass between the component needles and the escape of the similarly enclosed vapour which would cause contraction in a lateral direction, so that

[1] Prof. G. A. T. Cole and Mr. G. W. Butler, the authors of a recent paper "On the Lithophyses in the Obsidian of the Rocche Rosse, Lipari" (Q.J.G.S. August, 1892), having found that Dr. Johnston-Lavis had been led to an interpretation of the facts considered, different from theirs, and hitherto unpublished, asked him to add to the discussion of the matter a brief abstract of his views. In accordance with his suggestion, his reply is here published.—Edit. Geol. Mag.

the cones would tend to become more acute. Crystallization at the centre of the spherulite would be fairly complete, but as growth took place the supersaturation of the surrounding glass would tend to cause much of such glass to remain between the fibres unindividualized; the outer surface fibres would again jam tight on account of the relief of solution-tension by vesiculation. Thus there would be more interfibrillar glass in the middle zone and more contraction in that region when the spherulite breaks up. The chief cause, however, of the concave sides to many of the cones is that they are sectionized obliquely, and assume the concave-sided look through the microscope and an appreciable thickness of the section.

After vesiculation, growth continues normal to the vesicle wall and therefore no longer parallel with the fibres of the conical wedge, hence the mushroom or fan-like appearance.

Sometimes this wedge or cone is pushed into the glass by the expanding vapour behind it, as in your fig. 3.

You will see that I differ from you in holding that not only large part, but all of the felspar needles have developed *from within outwards.*

Of course I consider that tridimite, fayalite, magnetite, haematite, are true sublimates. If spherulites assume large size they become lithophysal for a second, third, and so on, spherical shells of vapour form around the new shell of spherulitic matter, and repeat a similar process to what occurred in the nucleus. Your figures are very good and characteristic. I interpret them as follows:—

Fig. 1.—Rapid and intense vesiculation around very small spherulites, which in the upper cavity have grown out a little, having for the most part been reduced to fine dust spread over the bubble walls. In the other vesicles the expansion and cooling was so rapid that no further growth could take place from the minute cones attached to the walls.

Fig. 2.—Well shows the broken-up cones and the difference in their circumference curve and that of the enlarged vesicle and the independent direction of the new formed fibres.

Fig. 3.—Shows one cone holding remains of nucleus of spherulite. It has been pushed into the wall of the vesicle by the expansion of the vapours on the opposite side.

Fig. 4.—Shows the union of one vesicle around several spherulites.

These phenomena occur only where expansion can take place from slight pressure.

The difference between my interpretation and yours seems to lie essentially in this—that your paper explains the universal association, in your specimens of cavities and spherulitic growths, as due to great part at any rate of these growths in the Lipari Obsidians having been *initiated* by the formation of steam vesicles; while I hold that such association may be attributed—(1) to the liberation of steam *during* crystallization; and (2) to the fact that the crystalline aggregates (spherulites) *when formed* would afford surfaces favourable to the disengagement of gas from the surrounding glass.

It is not only towards the *surface* of the Rocche Rosse lava stream

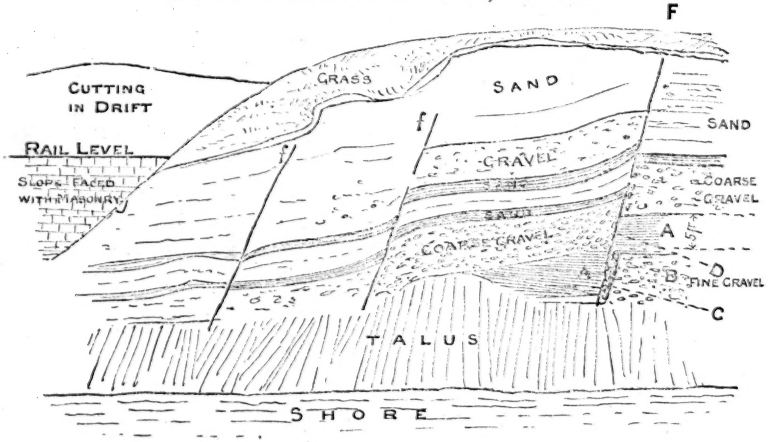
that the spherulites are plentiful, and this I consider to be opposed to your interpretation. I hope soon to be able to show you some basic spherulites of another character, which also throw light on the subject.

IV.—Faulting in Drift.

By T. Mellard Reade, C.E., F.G.S., F.R.I.B.A.

In the Geological Magazine of last year[1] I described a system of Miniature Faulting observed by me in a fine bed of banded silty clay at Nevin, Carnarvonshire. This year I have had the good fortune to observe Faulting in Drift on a much larger scale in the neighbourhood of St. Bees, Cumberland, which is highly instructive and explanatory of several features of Normal Faulting which the other illustration did not touch.

Section of Faulted Drift in Sea-cliff, Cumberland.



A. A. Bed of Laminated Sand displaced 5 ft by Fault F.

The Faulted Drift in question, as exhibited in the sketch, is exposed in a sea-cliff section South of Nethertown Station. It consists of various beds of gravels and sand, some of the latter being well laminated. The faulting occurs at the northern end of the section. It consists of one main fault (*F*) at right angles to the shore, and two other smaller and parallel faults (*ff*) stepped down to the north of it. In consequence of the sharp shearing of the beds, I was enabled to measure the throw, and found that the laminated sand $A = A$ on opposite sides of the fault showed a throw of five feet. The hade of the fault, which was at about the angle shown on the sketch, was to the down-throw, and the beds were turned up against the fault on the down-throw side. Not only so, but the gravel on the plane of the fault (*D*) was turned up with the long axes of the stones parallel to the hade. The distinction

[1] "A Miniature Illustration of Normal Faulting," Geol. Mag. Nov. 1891, p. 487.

between the gravel and the laminated sand where they abutted on each other on the fault-plane was as sharp as if the beds had been cut with a knife. In the case of the bed of fine gravel (*B*), the fault showed very prominently as the gravel projected some inches from the face of the cliff in a band about three inches wide (*C*) in which the pebbles were arranged with their long axes nearly vertical and parallel to the fault-plane, just like the fault-rock in more coherent material. The faulting is no doubt confined to the drift, and does not affect the bed rock, which is Permian sandstone.

Its cause was not obvious, and it was raining so hard at the time that I had no opportunity of making extended observations. The Drift has adjusted itself to different conditions of space from that in which it was laid down; but the chief lesson to be learned is the *mode* in which this adjustment usually takes place, *i.e.*, by shearing, even where the material is of so incoherent a nature as gravel and sand.

V.—Glacial Geology, Old and New.

By Percy F. Kendall, F.G.S.,
Lecturer on Geology at Yorkshire College, Leeds.

THE recent contribution of Mr. Reade to Glacial Geology (Geol. Mag. July, 1892, p. 310, *et seq.*) is a challenge which I take up in default of a worthier champion.

The origin of the drift deposits of Lancashire is not a question of sand-grains. Mr. Reade's observations upon those objects are a useful continuation of the work of the late John Arthur Phillips, but their bearing upon the rival theories is admittedly very remote. If the sand-grains are of directly marine origin (which is by no means certain, for the author makes no mention of derivation from the New Red Sandstone) they are just what one would expect in association with sea shells on either hypothesis.

Instead of the discussion of such rather irrelevant topics, Mr. Reade should give us a clear exposition of his views regarding 1. The sequence of events in Britain during Glacial times. 2. The limits of the submerged areas. 3. The sources of the glaciers which gave origin to the icebergs carrying *e.g.* the Criffel granite to Moel Tryfaen. 4. Mode of origin of Boulder-clay, especially of the intensely hard Till utterly devoid of stratification which is sometimes met with. 5. The mode of production of the striæ found so commonly on the rock-surfaces. 6. The mode of origin of the "ground-moraine" which he has described. 7. The distribution of life in his supposititious glacial sea. One or two of these could perhaps be conveniently discussed in the pages of the Geological Magazine, but to deal with the whole of them with a detailed disquisition upon Gloppa, Moel Tryfaen, etc., would require that the Editor, whose obliging spirit is so well known, should lease his journal and editorial chair to the Glacialists for a couple of years, and allow them to bring out as many double numbers as they pleased. I would remind Mr. Reade of the proverbial disproportion between the time required respectively for questions

and for answers, a disproportion intensified when the questioner possesses the ability of Mr. Reade. I will address myself to two points, and, having stated the facts so far as I can ascertain them by personal observation and by reading all the literature available to me, I will offer a few words by way of contrasting the different theories advanced to account for the phenomena. I purpose dealing, firstly, with the distribution of erratics in Lancashire, Cheshire, and the adjacent counties, and secondly, with the nature and distribution of molluscan remains found in the Anglo-Welsh Drift.

I. *The Distribution of Erratics.*

The erratics of the region I have selected are divisible into two tolerably well-defined groups—

a. Stones of foreign origin.

b. Stones of demonstrably local origin.

The former category includes the Granitic Rocks, Lavas, and ashes of the *Western side* of the Lake District, with a few stragglers from the Eastern side, such as the odd boulder of Shap granite found by Mr. Reade on Moel Tryfaen, and the sparse trail of blocks of the same rock traced by Mackintosh, Tiddeman, De Rance, and others, from the angle of Morecambe Bay in a curved line round *via* Longridge to Whalley. It also includes numerous examples of the granites, grits, and other well-marked types of rock found in Galloway and the neighbourhood of Criffel. Some half-dozen pebbles have also been recorded by Mr. Lomas of the remarkable Eurite of Ailsa Craig.[1] Flints also occur co-extensively with the other foreign rocks, and it has been assumed that they are necessarily of Irish origin, but from the fact that they are almost invariably of small size and much waterworn, I long ago inferred that they were immediately derived from some bed of river-gravel which had lain in the path of the ice which brought them into Lancashire. Whether their more remote origin was in Antrim, in the submarine extension of the Antrim Chalk, or in some other submerged outcrop of chalk, such as that which Mr. Goodchild supposes to exist at the mouth of the Solway Frith, matters not at all. If they come from a river-gravel the precise spot whence they were derived is immaterial.

With this enumeration the list of foreign rocks is completed. It is true that Dr. Hatch[2] thinks he has recognized Dolerites from Mull! but he gives no special locality in Mull, and from an extensive experience of Mull rocks I have no hesitation in saying that it is utterly futile to attempt a generic diagnosis.

Petrographers should remember the Anglesea Picrite and beware of confident identification of basic or ultrabasic Igneous rocks.

Near the mouth of the Vale of Llangollen, and thence south eastward, Welsh rocks make their appearance, and of these and of their peculiar distribution due account must be taken.

This mere cataloguing gives no idea of the *grouping* of the boulders, a subject which has apparently received less of Mr. Reade's attention

[1] Brit. Assoc. Report, 1892. See also Proc. Manch. Lit. & Phil. Soc. Feb. 1891.

[2] Brit. Assoc. Report, 1890, and Morton's Geol. of Liverpool, 2nd edition.

than it deserves. The careful surveys made by Messrs. Lomas, Dwerryhouse, Platt,[1] Mackintosh, and others, bring out very clearly the great preponderance of Lake District rocks on the eastern side of the area and of Scottish rocks on the western side.

The transport of local stones is easy of definition over the major part of the area. There is a small number of rocks in South Lancashire and Cheshire whose outcrops are so restricted that peculiarities of their distribution can be studied with great facility. I take three examples.

1. Thin Permian Limestones, crowded with *Bakevellia*, crop out along the northern edge of the great Permo-Triassic basin near Leigh and Patricroft, Lancashire. Boulders of the rock are found at Stickins Island, on the Manchester Ship Canal. Another outcrop runs through Manchester-Salford, and boulders appear on the eastern side. A third outcrop has been discovered in the neighbourhood of Stockport, and again to the eastward boulders of the rock appear in the Drift deposits. I personally drew the attention of a party of the Liverpool Geological Society to the Manchester Ship Canal specimens, and one and all declared they had never seen such a rock in the Liverpool district.

2. In the Upper Coal-measures of the Manchester Coalfield, peculiar limestones crowded with *Spirorbis* occur. The outcrops are very restricted, but run in a N. and S. line, down to about Heaton Chapel, near Manchester. Now, not only have we (and Mr. Reade is amongst the observers) seen in a long, open section streams of boulders trailing away through the Boulder-clay to the eastward from each outcrop, but sporadic boulders have been found at Apethorne Mill, Hyde; at Buckley's Mill, Woodley, and at Glossop. Not one scrap has been found to the northward or westward of the parent mass.

The Millstone-grit boulders display the same peculiarities. In the Liverpool district admirable work is being done in the mapping of Boulders, and in the report on Erratic Blocks presented to the Edinburgh meeting of the British Association, a series of upwards of 400, being *all* those visible along several miles of the Mersey shore-line, has been recorded by Messrs. Lomas and Dwerryhouse, and among them there is not a single block of Millstone-grit. A similar list for the Rochdale area, by Mr. Platt, shows a very high percentage of boulders of that rock. In my own experience in south Lancashire and Cheshire I have met with but two, viz., one in Manchester (Denmark Road), and one at Portwood, Stockport.

What is the reason for this very partial distribution? The "anti-submerger" would point to the fact that the ice-movement has been from the N.W., and that whereas to the N.W. of Hale Head there is no Millstone-grit above sea-level, there is a very large outcrop of the rock to the N.W. of Rochdale.

These and other cognate facts were in my mind when I declared in a paper quoted by Dr. Crosskey, in the 18th Report of the British Association Committee on Erratic Blocks: "Boulders in this district

[1] Brit. Assoc. Report on Erratic Blocks, 1890–92.

never occur either to the N. or W. of the parent rock. . . . . It is, I believe, the law of boulder-transport for south Lancashire and Cheshire." This statement has never been traversed, and Mr. Reade has nothing to urge against it except his vague and contradictory theory of the transport of the material of the Drift in the direction of the existing drainage. (By the way, how does Mr. Reade account for the Weaver producing the Widnes clays, which are in the valley of the Mersey several miles *above* its confluence with the Weaver?)

The facts given above may, I think, be best explained by supposing that a great lobe of ice came in from the Irish Sea, pushed over the low grounds of Lancashire, Cheshire, and Shropshire, and had its final melting place in the neighbourhood of Bridgenorth and Wolverhampton, where an amazing profusion of northern rocks—the "overshot load" of Mackintosh—and great piles of gravel and sand mark its termination. Upon its left flank, in the upper part of its course it was confluent with ice coming from the Lake District, and along the line of composition the thin trail of Shap Granite before alluded to is scattered. To the southward of Bacup it followed a line sub-parallel with the Pennine axis, which, however, it never crossed. On the right flank it rested against the outer line of hills of the great Welsh cluster, Halkin Mountain, Hope Mountain, Frondeg, and Gloppa.

Within the area thus roughly defined, *all* the indications point to a movement in the direction I have mentioned, saving only the few boulders of which Mr. Reade speaks as indicating a movement *from*, not *to*, the S.E. I deal with them later. The striæ exhibit a uniformity of direction surprising even to the advocates of the ice-sheet theory. In agreement with them is the orientation of large boulders, the direction of their sharper ends, the directions of Crag and Tail, the "forced arrangement" of boulders, the transport of local boulders, terminal curvature, and even, as Mr. De Rance, and later, Mr. Strahan, has remarked, the false-bedding of the Drift sands and gravels.

Mr. Reade, will hardly need to be reminded that such uniformity is not a characteristic of transport by tidal or drift currents, such as must be the main agents in the propulsion of icebergs.

Now, to consider the supposed cases of reversal of the direction of transport:—If we eliminate the flints, which, in the complete absence of evidence to the contrary, I can as well say came from the N.W., as Mr. Reade can assign to a south-easterly source, there remain some *eleven* exceptions to the "law," several of which are not in the area at all, and regarding nearly the whole of which, without any special pleading, it may safely be said that they are at least as likely to conform to the rule as not.

Of the eleven we have Lias accounting for four, viz.: "Lias" at Woollerton, Shropshire, "Lias" at Gloppa, "Lias Gryphites" near Ludlow, and broken Liassic fossils near Wolverhampton. Now, besides the known, though imperfectly mapped patch of Lias near Market Drayton, there is an outcrop in the north, near Carlisle, and before these exceptions can be accepted as valid, it must be shown that neither the one nor the other could have yielded them. Of

the Liassic Gault and Chalk fossils recorded from Gloppa Mr. Nicholson found one Liassic fossil himself, while the remainder were obtained from the workmen.

We are told that Mr. Nicholson "has no reason to doubt their genuineness." I fancy there are very few geologists who will share this confidence in the accuracy of the British navvy. The gangs are recruited from all parts of the country, they have a great aptitude for turning a penny, and the way they carry specimens from one job to another is matter of notoriety; but, by way of illustration, I will mention two out of many cases within my own experience. At Moel Tryfaen I was offered a West Indian *Pyrula*, and several other obviously recent shells, by a workman, who protested that he had found them in the famous Drift deposits; and in the cutting of the Levenshulme railway, when I asked an intelligent ganger if he had found any shells in the Boulder-clay, he produced from his pocket two well-preserved specimens, *Fusus bulbiformis* and *Turritella sulcifera!* Need I say that I should want better evidence before I could admit the occurrence of the Gault *Inoceramus concentricus* in the Gloppa gravels. It is not surprising that Mr. Nicholson, in his maiden effort, of which he has such good cause to be proud, should manifest a willingness to accept the testimony of the navvy, but in a geologist of Mr. Reade's experience such credulity is wonderful. Let me ask Mr. Reade how he would get Gault fossils (from the condition of the specimen I should say it came from the Cambridge Greensand) round and *up* to Gloppa by the agency of floating ice? The twists and doubles would need to be of a very complicated description, and when they were all performed a vertical rise of close upon 1000 feet would have to be effected.

The "exceptions" have now nearly reached the irreducible minimum by this process of elimination, but I must whittle them down a little farther. The coal at Corwen found by Mr. Mackintosh must be accepted by all who know anything of the work of that most conscientious and painstaking observer. But, though I unreservedly accept the fact, I must demur to the inference that the coal came from Ruabon. Up the Vale of Llangollen, though I have found a round dozen of moraines of a glacier which came down the Vale, neither I, nor, so far as I can learn from his writings, Mr. Mackintosh ever found the slightest trace of any movement or of any transport up the Vale from Ruabon in the direction of Corwen. I am writing away from my books and papers, and therefore do not speak positively upon the point, but I have some recollection of seeing plant-remains in the state of coal which were found by Dr. Hicks in the slates at Corwen. If this be the case it would suggest a much more probable origin for the coaly fragments than Ruabon.

The occurrence of *Waldheimia obovata* at Wellington is avouched by Dr. Callaway, and therefore is inexpugnable; by the ice-sheet theory, however, there is still an explanation without calling in the aid of a capping of Oolite in the Liassic basin of Market Drayton. The central cross valley of England through which the Trent runs was the site, according to Lewis, of a large extra-morainic lake,

produced by the obstruction by glacier-ice of the lower part of the Trent valley; another such obstruction, it appears probable, headed back the lower reaches of the Severn (upon this point I have formed no definite opinion), and the area of this lake was everywhere characterized in Lewis's words by a "commingling of the drift;" hence the Charnwood rocks at Nottingham, the Oolitic rocks at Shiffnal, the flints at Hanbury Woodend, and, perhaps, by the overflow of such a lake, the *Waldheimia* at Wellington. (I might add, too, for Mr. Reade's assistance in studying the problems, the Red Chalk recorded by Mr. Lucy in his paper on the "Gravels of the Severn, Avon, and Evenlode," and by Buckland in an earlier paper.) There remains now *one* errant erratic to be noticed, viz the pre-Cambrian rocks of the Wrekin, found by Mr. Reade "immediately to the north of the Wrekin." Mr. Reade should here be a little more explicit regarding his solitary piece of good evidence. What pre-Cambrian rock is it? In what sense does he use the word "immediately"? If the rock be a volcanic (Uriconian) one, then I would point out that the pre-Cambrian lavas extend some distance to the northward of the Wrekin; on the other hand, the granitoid rocks (Malvernian) do not occur in the Wrekin at all. When Mr. Reade answers these questions it will be time enough to decide whether of the eleven cases of abnormal transport there is, or is not, just one which is inexplicable on the ice-sheet hypothesis.

If this solitary exception will really stand criticism, I still think that until it is reinforced by a good many other examples in other parts of the north-west it can hardly justify us in postulating the submergence of about half the land area of the British Isles. I would far rather call in the aid of melting snows upon the steep flanks of Ercal itself than take so great a liberty with physical geography to effect so small a result.

Can Mr. Reade not recall from his twenty years' experience of the Drift round Liverpool (my Drift experience is barely five years old) a few, nay even one, good and authentic illustration of a S. to N. transport of a boulder? Has he never found so much as one unmistakable erratic, say, for example, the Penmaenmaur Diorite, or the granitic rock of Yr Eifl?

I now come to the consideration of the second class of evidence, viz.:

II. *The Nature and Distribution of the Molluscan Remains found in the Anglo-Welsh Drift.*

I have carefully investigated every recorded occurrence of marine shells in the Drift deposits of South Britain, and I find not a single exception to the rule that they lie directly in the course of ice which, by all the physical indications such as striæ, transport of boulders, and the like, can be shown to have come in upon the land from seaward.

It follows from this that all shelly drift should contain what may be called "transmarine" erratics. Two cases were recorded where there was a default of evidence on this point or an explicit declaration of their absence. In the first case (Gloppa) foreign stones had

but to be looked for to be found in profusion, and in the second case (Whalley),[1] where "obscure shell fragments" were recorded, a very short search sufficed to find foreign stones in the very same pit, and boulders, large and small, of Scottish and Cumbrian rocks were in evidence in many places in the neighbourhood. It is an exceptional thing to find any considerable deposit of Drift in the course of the great Irish Sea glacier, which is destitute of shells or shell fragments.

These are the positive facts of distribution. Now I would dwell a little on the negative evidence. I am duly mindful of the treacherous nature of negative evidence, but when able and industrious observers in all parts of the country have been assiduously searching for half a century or more, and have not brought forward one contradictory fact, I shall scarcely be accused of rashness if I lay some stress upon this failure.

If our country had been submerged to a depth of 1350 feet, it is surely not too much to ask why there are no shell-beds in the secluded mountain valleys in the great hill-clusters.

The fewness of the exposures, a reason assigned by Mr. Reade, cannot be taken as a very satisfactory explanation. If there are not so many gravel-pits or brickfields there are more mines and quarries, and the streams cut deeper and show cleaner sections; but, after all, it is not solely a question of shells, there should be beaches and cliffs, but where are they?

When we consider, again, the *condition* of the shells what do we find? The shells are comminuted and pounded in such a fashion that one may work for hours even in some of the most prolific beds and not find a perfect shell of any sort, and, astounding fact, in the 20,000 square miles or so of recently elevated sea-bottom in Lancashire, Cheshire, and the Drift-covered areas to the south, a bivalve with the valves in apposition has never been found, unless we admit as exceptions Mr. Nicholson's Gault *Inoceramus* and the *Saxicava* found in a bored boulder.

It might, too, be pointed out that we have no record of a single stone or shell being found encrusted with barnacles, and of the *two* bored stones recorded from the Boulder-clay of the area in question one was distinctly glaciated,[2] and the other, which formed the subject of a paper by Mr. Shone, had the crypts filled, not with the surrounding Boulder-clay, but with a fine sand very rich in microzoa, which is without its like amongst the Drift deposits of the district. Mr. Shone was led by this fortunate find to investigate the contents of univalves, and discovered that many of those found in the Boulder-clay contained a similar rich sand. He offered an explanation, which however ingenious, still fails to account for the total absence of any similar deposit on a larger scale. Scratched shell fragments are fairly common, and it is worthy of note that sub-perfect univalves are much more common than bivalves in equally good condition. The relative perfection of shells at high- and at low-levels respectively has been stated in the following very misleading terms (GEOL.

[1] See Geol. Surv. Memoir of the country round Burnley.

[2] "Nature," vol. xl. p. 246.

Mag. July, 1892, p. 312): "More perfect shells have been found at these high levels, especially at Gloppa, by Mr. Nicholson, F.G.S., than are generally met with in the Drift of the plains."

The plain English of this is "at certain selected localities at high-levels the shells are better preserved than in the average of low-level deposits." As a matter of fact, the low-level deposits include Leyland, which has yielded the largest list of shells ever compiled from an English or Welsh Drift-bed, and in point of mere numbers, I would undertake to collect more shells in an hour at Shellag Point, Isle of Man, than Mr. Reade could in a day at Gloppa, or a month at Moel Tryfaen. But as a matter of fact, there is, in the N.W. of England, no such thing as a glacial shell-*bed*. Anyone coming fresh from the old sea-beaches and sea-bottoms of East Anglia would, I doubt not, regard with astonishment the contents of these so-called marine deposits.

I turn now to the consideration of the nature of the fauna, and any remarks of a general nature which I may make must be taken to apply solely to the country to the westward of the Pennine Chain.

The shells throughout the whole area are, I consider, such as could not have existed in British Seas during the Glacial period. Mr. Reade, many years ago, remarked that the post-Glacial beds of the Clyde contained a fauna much more indicative of severe conditions than that contained in the Drift of the North-west of England. The fauna is not only an inconsistent one in respect of the mixture of shells of diverse habitat, warm-water and cold-water forms, sand and rock-haunting species with mud-loving forms, but, as Forbes pointed out 46 years ago, there is *an utter absence of deep-water fauna*. He says[1] "That in no case, so far as I have examined, the upheaved strata were formed under conditions of considerable depth, such as my *region of deep-sea corals* [50 fathoms to beyond 100] now presents, is rendered almost certain by the total absence of the remains of the characteristic inhabitants of that region." Again, "So far as I have seen, there is no British case of an upheaved stratum containing organic remains evidently untransported which may not have been formed at a less depth than 25 fathoms . . . . and it is probable that between 10 and 15 fathoms would more frequently approach the truth."

Mr. Reade has repeatedly commented upon the general similarity of the faunæ, and on p. 312 of the paper I am criticizing, he says—"These sands and gravels [*i.e.* at 1000–1400 feet above the sea-level] contain shells of mollusca, speaking generally, of a similar *facies* to those fragments found in the low-level Boulder-clay and sands." (The "fragments" is refreshing.) Now has Mr. Reade ever considered that with a submergence of 1400 feet there would have been a depth of *two hundred and thirty-three fathoms* over the low grounds of Lancashire, and will he be bold enough to say that *Turritella terebra*, *Purpura lapillus*, *Tellina balthica*, and *Cardium edule*, form the natural assemblage of shells which one should find *on a muddy bottom in* 200 *fathoms of water?*

[1] Mem. Geol. Survey, vol. i. p. 376.

The attempt to explain away the occurrence of southern forms by such remarks as that *Cytherea chione* is the only one of "anything approaching a wide distribution," is in the first place hardly correct, as *Arca lactea* has been repeatedly found; but, if it were, it is a very damaging admission. *Cytherea chione* is most distinctly a southern shell, and though it occurs in Carnarvon Bay I have heard of but two examples being found, viz.: one dredged by Mr. MacAndrew, many years ago, and the other, by myself, in a very "dead" condition. The statement that "some Tertiary shells are now only boreal in their habitat, notably *Tellina calcarea*," and the footnote quotation from Jeffreys, "Occurs in every Tertiary bed up to the Red Crag," reveals one of the dangers to which the geologists of the north-west are peculiarly liable. They are out of reach of any Tertiary beds except the Pleistocene, and hence are compelled to take some of their geology, when it relates to Tertiaries, as well as their conchology, from Jeffreys.

The only way in which Jeffreys' assertion could be made to square with the facts is to turn the geological succession upside down; then, and then only, could one say that *Tellina calcarea* occurs in every Tertiary bed *up* to the Red Crag.

I say, without fear of contradiction, that it does not occur *below* the Red Crag, and that even its occurrence in that deposit is very questionable. For my own part I do not admit the identity of *T. calcarea* and the Red Crag *T. pretenuis*. Furthermore, I assert, and challenge refutation, that a large portion of the Tertiary Period, to wit, the latter part of the Red Crag and subsequent Tertiary times, including the Chillesford and Forest Bed epochs, were characterized by a climate distinctly "colder than the present." Mr. Reade holds a contrary view, but I am satisfied that he is singular in his opinion.

It remains now for me to notice the most remarkable of all the shells of the Drift, viz., the extinct species. These have not received the attention they deserve, as the temptation is so great to "locate" a species as nearly as one can, instead of laboriously hunting out its precise affinities. Strange to say, Edward Forbes in this way overlooked the most remarkable shell in the Manx drift, viz., *Nassa reticosa*, Sow. (=*N. serrata*, Broc.), which he identified as "*Buccinum undatum* var., resembling *Nassa reticosa*." This shell is extremely abundant in the Red Crag, and less so in the Coralline, but, in East Anglia, it does not appear to have survived the close of the Red Crag period.

In the Manx Drift it is very abundant, and is associated with *Columbella sulcata*, and *Nassa monensis*, Red Crag species, and *Fusus Forbesii*, a large and handsome species, which is not known elsewhere either recent or fossil. A *Mitra* also occurs, which is either extinct or a southern species. In the Wexford Drift *N. reticosa* is found, together with *Melampus pyramidalis* (otherwise confined to the Pliocenes of East Anglia and Cornwall), a *Mitra*, and *Turritella triplicata*, Broc. (=*T. incrassata*, Sow.), a species very abundant in British seas in the early part of the Pliocene period, but now

banished to the Mediterranean. Other shells of equal interest could be added, but space forbids.

How shall we interpret these facts? Are they best explained by supposing the shells to have inhabited a sea which had, albeit over 200 fathoms deep, no deep-water fauna at all, which nowhere left a shell-bank or even a group of shells on the spot where they lived (on this point Gwyn Jeffreys and Mr. R. D. Darbishire spoke very explicitly), which mingled shells of the most diverse habitat and left shells at present confined to shallow water and a sandy bottom in the muds of its profoundest depths, a sea which pertinaciously avoided the deep gulfs between the tiny islets that studded its surface, and that finally retired without leaving beach, shore-platform, or so much as a cliff or sea-worn cave behind to mark its former extension.

Or, do not the facts harmonize better with the view, so strongly supported by the physical indications, that the ice-sheet, whose extension over the district and in the direction I postulate is admitted by Mr. Reade,[1] having advanced over the Irish Sea, bore along involved in its mass or in its ground-moraine portions of the vast banks of shells with which that, then as now, *shallow* sea was cumbered, and mixed up the mangled remains, not only of the but-recently-vacated boreal forms, but also of the warm-water species which lived in the preceding Pliocene Period.

In conclusion I would remark that it is before all things necessary, in the discussion of the question of the condition of Britain during the Glacial Period, to take a pretty wide survey of the country, and to try and bring all the facts into a single *coup d'œil*. The former Mr. Reade has certainly done, as witness his papers on the Drift of Cromer and Yorkshire; but the latter I fear he has not attempted, else we should hear less of the significance of the fact that Three Rock Mountain, Moel Tryfaen, Gloppa, and Macclesfield are at about the same altitude and on the same parallel of latitude. Let Mr. Reade look 20 or 30 miles E. and W. of his terminals and see in what way his great submergence declined.

I have touched but upon the fringe of a great subject, and must leave the publication of a more general statement of my views till the autumn, when Prof. G. F. Wright's new volume in the International Scientific Series, to which I have contributed a chapter, will be out. In the meantime Mr. Reade will, I hope, deal with some of the topics which I suggested at the outset.

VI.—Selenology.

By S. E. Peal, Esq.

As one who has for many years made a special study of the lunar surface, and who is thoroughly convinced that the geologist alone can now extricate selenology from the slough in which it has hopelessly stuck fast, I venture very briefly to lay the case before your readers, in the hope that some one will come forward and help us.

[1] See his papers in the Q.J.G.S., and "Nature," vol. xl. p. 247.

As regards its distance, motions, and topography, or mapping of the surface details, our knowledge of the moon is well to the fore, every little mound and fault being carefully recorded; but what the surface, so clearly seen, is composed of—whether of volcanic or stratified rocks, or otherwise—no one can say.

The structure and material of the surface is in fact a most hopeless enigma, and this is all the more extraordinary when we recollect that of all the heavenly host, our moon is the nearest body, and that most frequently and easily seen, being totally unobscured by atmosphere or clouds, and easily observed with small telescopes and low powers.

To some extent this want of progress in selenology is due to the old idea that the surface is now covered from pole to pole with the remains of lava-lakes and stupendous volcanic explosions, a vast cinder heap in fact, and is also partly due to the researches of Lord Rosse, which (erroneously as we now know) were held to demonstrate a maximum temperature of + 300° C. after 14 days' sunshine.

But our want of progress is also undoubtedly due in part to the general belief among astronomers, that a planet like our moon could actually pass from the semi-incandescent and lava-crusted stage, with huge vaporous envelope, direct to the cold, airless and waterless stage, without an era of erosion intervening.

Could retain, that is, its primeval volcanic surfacing throughout the long era succeeding, while the temperature slowly declined, and without the intervention of an era of erosion and deposition of sedimentary rocks, as on our Earth.

Judging by our own vast series of stratified rocks, we are led to conclude that an exceedingly long temperate 'era of erosion' must, in the very nature of things, supervene on the heated lava-lake stage, in all planetary development.

It is over this part of the question that I invoke the aid of your readers, *i.e.* to say, as geologists, whether they believe that a planet, such as our moon, could retain from the igneous-molten era, to its present *intensely* cold, airless and waterless condition, its pristine surfacing, the very poles themselves being covered (it is urged) with large and small volcanoes, untouched by the hand of time.

The experts who have specially studied the question, agree that the maximum surface temperature under 14 days' solar heat, is at or about zero, C.; and the minimum during the lunar night falls to about — 200° C.; the mean being so low that solar heat raises no trace of vapour, about the equator, at the limb.

Thus the surface temperature of the moon must have fallen enormously, and taken millions of years to do so.

To aid your geological readers who may not be aware of the peculiarities of the case I would point out that—

1. The moon (unlike Mars) has no polar caps.

2. There is an absence of all *distinct* colour in masses or in detail. Over the entire surface we find warm greys and neutral tints prevailing.

3. There is a general and conspicuous absence of all evidence of

drainage, and river valleys. The surface of the planet is literally covered with unearthly circular formations, from 10 to 100 or even 150 miles in diameter, enclosing level plains sunk from 1000 to 10,000 feet beneath the outer surface, and bordered by vast ramparts, having about the same cubic measurement as that of the excavation.

4. The mountains, peaks, and cliffs are white, and *whitest* where they are *steepest*. The darker parts of the surface are levels, as though due to accumulation of meteoric dust thereon.

5. There is an entire absence of *raised* level floors like those in terrestrial volcanoes, and there is no perceptible difference whatever between the polar and equatorial surfacing; if the large and small circular formations about the equator have been volcanoes, then have the lunar poles been finally surfaced in the same way.

Lastly, if, as is now generally admitted, the moon formerly rotated more rapidly on her axis, and has been slowed down by tidal friction, this would imply a temperate era, the existence of water, and of erosive action, which would have effectually scoured off all the so-called "volcanic" details now visible everywhere.

So far our moon, though so admirably suited in some ways for study by geologists, seems to have received rather scant attention. Professor Judd, in his most instructive work on Volcanoes, p. 305, says "the moon appears to be destitute of both atmosphere and water. Under these circumstances we find its surface, as we might expect, to be composed of rocks which appear to be entirely of igneous origin; the mountain-masses, unworn by rain or frost, river or glacier, being of most prodigious dimensions as compared with those of our own globe, while no feature at all resembling valleys, or plains, or alluvial flats are anywhere to be discerned upon the lunar surface."

The whole of Prof. Judd's work, however, is such a clear and beautiful demonstration, that without water there can be no volcano, that I feel sure he will excuse my pointing out the above difficulty.

With your permission I would like to say that if any reader desires to study my "Theory of Lunar Surfacing by Glaciation," it is to be had at Messrs. Thacker & Co.'s, 87, Newgate Street. I earnestly hope that some of your geological experts will take this matter in hand; it will certainly repay them, and perhaps enable selenology to make some advance, after prolonged stagnation.

Sibsagar, Assam, 11th June, 1892.

VII.—The Mammoth and the Glacial Drift. A Reply to Mr. A. J. Jukes-Browne.

By Sir Henry H. Howorth, K.C.S.I., M.P., F.G.S., etc.

In a letter in the last Number of the Geological Magazine, Mr. Jukes-Browne takes me to task, with some asperity of language, for my views on the true horizon of the Mammoth, You will, I am sure, allow me to reply to him. He speaks of my "looking down from some literary height upon practical geologists in the plain," and goes on to say that I have "no practical experience as a geologist, and bids me study the facts in the field."

This is certainly a sublime height from which I never addressed anybody. The language compels me to say, much to my distaste, that for many years past I have spent my holidays in trying to explore the so-called Glacial beds in Scotland, Eastern and Western England, Holland, North Germany, Denmark, the Scandinavian Peninsula, Finland, and Switzerland. It is possible, but it is not probable, that Mr. Jukes-Browne has studied them *in the field* in as many aspects and for as many years as I have.

Two lessons, *inter alia*, this examination has taught me, namely, the extreme difficulty of disentangling the history of these beds, and the impossibility of any individual explorer doing so unless, and until, he has mastered not only the facts in the field, which are comparatively easy to collect, but the vast and intricate literature dealing with the subject. I am not alone in this view. It is now some years since I paid my first visit to the Cromer Cliffs with two distinguished geological friends who knew them well. When we had finished our examination, and had concluded, as many others have concluded before, that the riddles they enshrine are not solved by any current theory about them, one of my friends, a geological surveyor, went on to say that what we needed far more than a continual accession of disintegrated facts, where facts are so abundant, was a sifting of the enormous literature relating to the so-called Glacial beds, so that we might marshal the testimony of the hundreds of observers and get rid of the personal equation which underlies them. I thought this a sensible remark, and I have probably devoted as much time to the very dreary work as anybody. One result will appear in the course of a few weeks in a work of 900 closely packed pages on what I have ventured to call the Glacial Nightmare. Another result I have tried to present in the pages of the Geological Magazine. Mr. Jukes-Browne affects to despise this kind of work, and yet I have no doubt he agrees with me that any man deserves condemnation who in these days, when so many busy hands and eyes are at work, ventures to publish as his own what others have published long before and consequently that no one has a right to publish a fact until he has taken great pains to find out that it is new and of some importance, as well as true.

What I have done, and shall continue to do, in any geological excursions I may make, is, not only to report my own limited experience in the field, but also to bring together, as far as my bad health and mortgaged time will allow, the varying and contradictory testimony of other explorers, many of them better men than myself, and to try and equate their testimony with some positive conclusion. In many cases it is impossible to re-examine the sections, since they are effaced, and if they were not, my own observations would not, in my eyes, be of greater authority than those testified to by others, whose judgment I respect. We all need checking, and all our testimony needs sifting, and nowhere more so than when stupendous issues depend upon the result.

I hold it to be of very great importance, not merely on the question

of settling the succession of the surface beds, but in tracing the beginnings of human life on the earth, that we should if we can, fix the horizon of the Mammoth beds. I have therefore applied what I hope is candid criticism to the evidence in regard to them, and the result is a conviction in my own mind that the evidence breaks down or is most unsatisfactory in every case which is supposed to prove that the Mammoth lived *after* the distribution of the drifts. Mr. Jukes-Browne says that I claim to have proved a universal negative, which is beyond human power. My object was very much more modest. It was merely to conclude from the *available*, not the *unavailable*, evidence that the case for the existence of the Mammoth after the distribution of the Drifts entirely breaks down. My conclusion was certainly not the result of *à priori* prejudice, for I have openly recanted some of my early views on this very subject, drawn from a partial and imperfect induction. That any one should be deemed guilty of anything but a service to science who subjects the discordant and divergent testimonies of "practical geologists" to criticism, is a curious instance of a reversion to ecclesiastical methods of discussion.

Mr. Jukes-Browne does not profess to meet my arguments or deny my conclusions, except in one instance, namely, that of Burgh, in Lincolnshire. This is a peculiarly unfortunate instance, because whatever views may have been held about the Mammoth having been an inter-glacial animal, I do not know any one who contends that the *E. antiquus* and the *R. leptorhinus*, both of which were found in the bed in question at Burgh, and both of which belong to an older horizon, can have lived during some interval in the so-called glacial age. Mr. Jukes-Browne's test of my capacity, namely, that I have mistaken one part of the valley of the Ouse for another, is, as your readers can see if they will turn to what I wrote, a test only of my critic's knowledge of the English language. I have nothing to correct, and nothing to alter in what I wrote, save the spelling of the word *remanié*, which was due to the printer's difficulty with my writing; but I emphatically re-assert my very strong objection to the basing of such a tremendous postulate as the intercalation of a Mammoth period between two ice ages, upon the broken down and utterly fragile evidence which is alone forthcoming to support it.

Lastly, Mr. Jukes-Browne, who apparently fancies that I am a wealthy man, instead of being a very poor one, bids me dig and test the case in a proper way, like my friend General Pitt-Rivers has tested other questions. I wish I could afford to dig, for I quite agree with him about the absolute necessity of digging, if we are to test the case properly; and I have said so very strongly at the two last anniversary dinners of the Geological Society. Nothing to me seems more futile and absurd than that a great public institution like the Geological Survey, should devote years of the work of some of the ablest men it can command, including Mr. Jukes-Browne, and expect them to report upon the most difficult beds in the world, and to do so and to map with no other evidence before them than

the sections in casual clay-pits or chalk-pits, or in the sea-cliffs; sections which have been read almost in as many ways as there have been explorers. No wonder that the results satisfy nobody, least of all (as I know in several cases) the reporters themselves. All this I quite agree in, and I hope if the Government will not find money, the British Association or the Royal Society will do so, in order that we may have some test digging. What I do object to is that Mr. Jukes-Browne should transfer the burden of proof to my shoulders, and that he should bid *me* dig, in order to supply the lack of evidence for *his* conclusions. The burden of my paper is to show that the case of those who affirm the so-called inter-glacial or post-glacial existence of the Mammoth completely breaks down when tested. It is, therefore, for the champions of that view to find some evidence to support it which is not entirely fly-blown. Those who ought to dig are those whose case is in jeopardy. No one would welcome such digging more than myself, whatever its results, for I never can understand the pique and temper which some men show when their views are no longer tenable, as if all human knowledge were not more or less tentative; and as if any sensible man values an hypothesis by its finality. Let Mr. Jukes-Browne, then, press on, with the help of us all, in the application of a real test to the evidence in question; and if we cannot have digging, let us at all events retain our scientific credit by applying adequate criticism to every statement and every fact upon which a wide-reaching conclusion is based.

NOTICES OF MEMOIRS.

I.—On a Sample of Cone-in-Cone Structure, found at Picton, New South Wales.[1] By A. J. Sach, F.C.S.

THE so-called Cone-in-Cone structure, which appears to be found in most countries, and which consists either of impure carbonate of lime or, less frequently, of impure carbonate of iron, still awaits a satisfactory explanation as to its mode of formation. It is more for the sake of eliciting the opinions of the geologists now assembled, than of advancing any theory of my own, that I exhibit the present specimen, and offer a few remarks on its composition and structure.

Out of some half-dozen of geological text-books that I consulted in the public libraries of Sydney, that by Sir Archibald Geikie, F.R.S., is the only one containing a reference to the Cone-in-Cone structure. Geikie appears to adopt the opinion of Professor Marsh, who states that the complex structure known as Cone-in-Cone may be due to the action of pressure upon concretions in the course of formation.

H. C. Sorby, F.R.S., in a paper read before the British Association, 1859, stated that he had examined transparent sections of the structure with a low magnifying power under polarized light, and concluded that it was intimately connected with some kind of Oolitic

[1] Read before the Australian Association for the Advancement of Science, Section C. (Geology); Hobart, 1892.

grains, which have crystals of calcium carbonate deposited almost entirely on one side along the axis of the cones in such a fan-shaped manner as to give rise to their conical shape. He states his conviction that the structure is one of the peculiar form of concretions formed after the deposition of the rock in which they occur by the crystallization of the calcium carbonate and other isomorphous bases.

Dr. Dawson, in his Acadian Geology, 1868, asserts that the structure is produced by concretionary action proceeding from the surface of a bed or layer, and modified by gradual compression of the material.

R. Daintree, F.G.S., Quarterly Journal of Geological Society, vol. xxviii. 1872, says that the structure has more of the appearance of a chemical precipitate than of a mechanical deposit.

John Young, F.G.S., Transactions of the Geological Society of Glasgow, vol. viii., read a paper on the subject in 1885. He possessed evidence that the band of Cone-in-Cone structure, which he described, rested on a clay-band ironstone, and that it was on the same horizon as a bed of stratified shale, composed in bulk of calcareous shells of Entomostraca, of species frequenting lacustrine waters. He possessed many samples of the mineral which had been found in Scotland, and none had been associated with marine deposits. After careful examination he concluded that the Cone-in-Cone structure is the result of a mechanical action set up by chemical agencies generated in the stratum, and whilst the deposition of the sediment was going on. The chemical agencies were the outward and upward escape of gases generated by the decomposition of organic matter in the deposit; the gases, as they escaped through the oozy and plastic mud, elevated the sediment around the several points of eruption into ring-like layers.

The sample which I now exhibit occurs at Picton, New South Wales, in the upper course of the Picton Creek, which traverses a valley locally known as Glenforsa. The hills on either side are well-grassed slopes of Wianamatta shales, which are of Triassic age, and are generally considered of fresh-water origin. I do not know of any extensive shell-beds or other lime deposit found in the shales, but when traversing the glen some irregular nodules of calcium carbonate were picked out of the banks of the creek. The Cone-in-Cone mineral occurs as a horizontal layer, which is exposed in the bed of the creek, but passes under the adjoining bank. So far as I could learn, it is not now in process of formation. The thickness is about two inches, composed entirely of cones within cones closely packed together. It has been asserted that in some European specimens the apices of the cones point both upward and downward, but in the specimen now under consideration all the apices point downward. The open bases of the cones, formed of ampitheatre-like cavities, are about half an inch in diameter, and small ones are sometimes formed within the larger ones. The chemical composition of the specimen is, approximately—Calcium carbonate, 67·54 per cent.; matter insoluble in strong hydrochloric acid, 21·2; sesquioxide of iron, 4·14; magnesium carbonate, 0·7; water, 3·1. In some parts

the mineral is distinctly crystalline, and, in my opinion, a purely mechanical origin can scarcely be entertained. It appears to be a chemical precipitate which has resulted in imperfect or disguised crystallization. The floor crystallizations, known as "crystal cities," at the Jenolan Caves, N.S.W., have a somewhat similar external form. The mineral might have been formed in the drying up of the calcareous waters of a lake.

PAPERS AND REPORTS READ BEFORE THE BRITISH ASSOCIATION, EDINBURGH, AUGUST, 1892.

II.—REPORT OF THE COMMITTEE, consisting of Messrs. H. BAUERMAN, F. W. RUDLER, and J. J. H. TEALL, and Dr. JOHNSTON-LAVIS, appointed for the investigation of the Volcanic Phenomena of Vesuvius and its Neighbourhood. (Drawn up by Dr. JOHNSTON-LAVIS.)

SINCE the last Report, nearly all the tunnelling for the great main sewer is complete, and few additional facts of interest have come to light. Several little problems of purely local geology have, however, been solved. In the lower sewer collector, beneath the tramway tunnel of Naples, a peculiar grey trachytic mass has been met with, and was penetrated for a short distance. On account of a lawsuit the works do not progress. The mass, however, is of considerable interest, as it is below the great yellow tuff of Posillipo and Naples. The rock was ejected rapidly in very pasty or almost solid fragments, which in some cases blended with the others thrown out just before and after, and are flattened out in a pipernoid manner. At other points the fragments are broken, mixed with dust, and consequently incoherent. When this deposit is cut through, it will probably confirm my theory regarding the piperno of Pianura and Soccavo, as being the result of lumps of lava ejected in great blobs, which, falling quite hot around the vent, have become resoldered together, and have even flowed a little.

Very high temperatures have been met with in the tunnel near the Solfatara, where I registered myself 59° C. on a day that the workmen considered a very fresh one for the workings, *i.e.*, with a high barometer still rising.

The statement made many years since by Professor A. Scacchi, that fragments of leucitic lavas had been found by him at the Foce di Fusaro, near the Torre Gavetta, led me to suspect the existence there of the Museum breccia. On examining the locality this was confirmed, and, in fact, the whole sea cliff of Mte. di Procida exhibits a most interesting, though complex, section of the volcanic series of the Phlegrean Fields.

We there have a series of trachytes forming the base of the section, very various in texture, and often covered with thick beds of their own scoriæ, which are often consolidated into a kind of trachytic breccia quite analogous to the 'sperrone' of the Alban hills. This is overlaid by fine lapilli and pumice beds, which vary very much in thickness. Lying unconformably upon these are irregular buried outliers of the grey pipernoid tuff of the region.

In one place the pipernoid tuff is seen in section as an exceedingly obtuse V-shaped mass, having choked an old valley, and possessing the following characters: The black scoria fragments are very slightly, if at all, flattened, are very spongy, are of good size, and form an important constituent of the deposit. From this we can conclude that the distance would well correspond with my supposed eruptive mouth to the S.S.W. of Camaldoli, not very far from the Lago d'Agnano, from which I believed issued the piperno and the greater part of the pipernoid tuff of the Campania. The distance was, in fact, such as to allow time for only the lighter pieces of scoriæ, the equivalent to the black flackers of the piperno, to travel so far, and these to be so cooled, that when they fell they were sufficiently rigid to no longer be flattened out by the impact.

This grey pipernoid tuff has here suffered much denudation, for in many places it is quite removed. Towards Torre Gavetta the 'Museum' breccia is well developed, being composed of very large blocks of the numerous varied rocks, followed by beds of the woody pumice, woody looking scoriæ, and scoriaceous black centred vitreous trachyte fragments and pumice. Lying with very marked unconformability upon it is a great thick bed of the compact yellow tuff, either derived from Campagnone or the neighbouring cone, a slice of which forms Misenum. In this section we have splendidly exhibited many of the great geological records in the history of this remarkable volcanic region. Each of these stages is defined from those above and below it by more or less long periods, during which, in some cases, very extensive denudation had taken place. At this point also I am satisfied we have products of the eruptions of the Procida and neighbouring centres interstratified with those of the mainland, and in which in time I hope to work out the relative chronology.

The discoveries, which in so striking a manner confirm my conclusions regarding the highly complex stratigraphy of this region, induced me once more to examine in detail that isolated eminence upon which once stood the renowned Greek town of Cumæ, founded about 1000 B.C.

Time has favoured the geologist, for here, however much the archæologist may grieve, it has once more exposed to human eyes sections that for many centuries were hidden by buildings, but which reveal the fact that those very rocks that, as geologists, we look upon as very recent, had nearly 3000 years since much the same characters as now. The pumices that form the uppermost yellow tuff had then already been converted into a rock that those early colonists cut out and used for the construction of their walls. When we first visit Cumæ, and our thoughts wander back through historic time, we are impressed by the human associations with this hill for such a long period; but when we return with our eyes and minds geologically cultured, the ancient Greek town sinks into insignificance by the side of the physical history of the mound it stood upon, when we remember that not only this mound, but the whole region is post-Pliocene in age.

The foundation of the Cumean hill is the well-known trachyte, rich in inclusions of sodalite, of amphibole, and I have detected, not uncommonly, crystals of fayalite. Were it more vesicular it would very much resemble the western mass of trachyte of the Cumana railway tunnel at the back of Naples.

Above this come some pumice and dust beds, which are probably the equivalent of the Rione Amedeo tuffs. Superposed on this we find a dirty grey pipernoid tuff which shows much *remaniement*. The Museum breccia is well represented in patches, and is overlaid by a bed of vitreous trachyte produced by the resoldering of the falling masses into one solid stratum, where the surface was flat, but where on an incline the fragments have remained separate. The whole is capped by the compact yellow tuff. There are also some minor pumice and dust beds which require further working out.

The trachyte seems to have oozed forth in a highly pasty condition, breaking up its scoriaceous surface, which rolled down the sides of the dome-shaped mass, and by pressure and heat from the main mass become again soldered together—in fact, a sort of regelation. The brecciated structure is undiscernible in hand specimens or under the microscope, but is well etched out by meteoric agencies. Each of the deposits mentioned above shows more or less unconformability, which correspond as they themselves do with those beds of the Monte Santo Funicular Railway, the Cumana Railway, Pianura, Soccavo, Monte di Procida, Nocera, Castellamare, St. Agata, Capri, Caserta, etc., that I have described in other reports and papers.

These are the principal sections which record the later geological history of the Phlegrean Fields, and from which I have been able to unravel the stratigraphy of the highly complex Neapolitan volcanic region. So far it has been explained only in these reports and other disjointed papers, but before long I hope to be able to place before the scientific world a far more detailed description of one of the most interesting as well as the most classic and accessible volcanic regions of the world.

Before quitting the subject, however, I wish to call attention to the confirmation that the sections mentioned in this Report afford of my explanation of the piperno and pipernoid structure in general. We see distinctly that the variation in colour and texture of the two constituents of the piperno, which chemically are identical, is simply due to the greater saturation with H_2O of one portion of the magma than the other in the old chimney of the volcano at the time of the eruption. The consequence was that the more aquiferous part was erupted as a fine dust, and the less aquiferous, more coherent magma was ejected in large fragments or more or less scoriaceous cakes, which lost their heat the more slowly, in proportion to the less water they contained. The densest, and at the same time the slowest to cool, fell near the eruptive mouth, flattened out, squeezed out those beneath them, and were squeezed out by those above them, forming, with the included dust, the compact piperno in which the foliated structure is most developed towards the west end of the

Soccavo section, where the nearest existing remnant to the old crater is now preserved, and where the inclination was greatest, and consequently where actually slight flow took place. The more scoriaceous of these lava cakes were carried to greater distances, so that as we travel away from the eruptive axis we find, first, that the black fragments become less markedly flattened because they cooled more rapidly from expansion, and also because they travelled farther, until they no longer show flattening parallel to the bedding more than what would be due to any of them being accidentally of a flattened or elongated form, and so lying flat on the surface they fell upon; second, we find that as their radial distance from the eruptive axis increased, the fragments at first get lighter; and third, when the limit of lightness and cohesion is reached, they get smaller and smaller, so that at Roccamonfina and Salerno the pipernoid tuff is chiefly composed of the grey dust with only few and minute fragments of black scoriæ. This seems to have been modified by strong winds and possibly by the eruption taking place along a cleft much like that formed in the late Tarawera eruption, or as in many cases in Iceland, such as the Skaptar outburst, though most of the latter locality does not belong to explosive types of eruptions.

There is in Iceland, at Krisuvik, the principal one of several crater lakes that exhibit in a striking manner the resoldering together of ejected fragments, into what might at first appear to be a true lava stream. I allude to the Groenavatn, in which we have an almost circular conical hollow nearly filled with water. There is only a very low ring round it, composed of *accidental* ejectamenta, being nothing more than the ejected fragments of the materials, through which it was drilled, with practically no essential ejecta, except on one side, where we have a mass of rock that looks like a lava stream. It seems there must have been at the moment of the eruption a very strong wind which carried all the lava fragments in one direction, and as they fell they blended together into one fairly uniform mass, the components of which are only faintly indicated by a slight variation in colour, somewhat like piperno, but not so well marked. The top and bottom are less coherent, for at the bottom the fragments fell on cold ground, whilst the top, although falling on the hot mass beneath, could not be pressed into contact with it by later falls. No doubt also the explosions were feebler towards the end and the interval longer between the fall of the last fragments.

We find exactly the same thing in the piperno, namely, a spongy tufaceous-like bottom and top. Besides this, the lulls and accentuations of the explosive action are well marked, as well as the time that large masses of the crater edges fell in and were re-ejected. At one time the eruptive action seems to have been arrested, and the partly or entirely consolidated plug was blown out into fragments and deposited amongst the piperno.

Vesuvius has since the last report, up to the time of my last visit in May, shown very little variation. It will be remembered that lava was issuing at the site of the eruption of June 7, 1891, at the

foot of the great cone, more than three hundred metres below the summit at the junction with the Atrio del Cavallo, and nearly opposite the Punta del Nasone. This outpour practically never stopped—at times it increased to no inconsiderable quantities, but flowed only a short distance, on account of the low gradient, tending to pile itself up into a mound. On other occasions it seemed to become almost arrested, but it never practically stopped. The consequence of all this was that at the foot of the great cone in the Atrio, during the year from June 1891 to June 1892, a tremendous mound or low-pitched buttress had been built up, so that its highest part I estimate to be 20 m. above the old floor of the Atrio. This thickening away in all directions, but even under the escarpment of Somma, the present floor stands for considerable distances over 5 m. higher. In consequence of this many of the dyke numbers which cost me so much labour to put up some years since have been covered over. These I hope to be able to replace this winter, and to repaint all the rest that are now becoming obliterated. It will be remembered that these numbers correspond with the dykes figured in my geological map of Vesuvius, and all collectors now adopt these numbers to indicate the locality of the dyke from which the specimens were obtained. Professor Bassani has added a new and complete collection of these interesting dyke rocks to the Naples Museum, and has arranged them according to my numbering. The great importance will be seen of maintaining this numbering intact.

The actual details of the variations in the activity are as follows. During the summer and autumn of 1891 more crumbling in of the crater edges took place, followed by black sand and dust-charged vapour. The outpour of lava from the base of the cone in the Atrio from time to time almost stopped, to be followed again by fresh gushes. On the first day of December a marked extension took place to the south and south-east of the crater by the further crumbling in of its edges. On the last day of the old year and commencement of 1892, the outflow was much accentuated. During January and February few variations were observable, but on March 17 and 18 slight reflection from the crater was visible for the first time for nearly a year, showing the rise of the lava in the chimney, due certainly in part from blocking of the lateral channel as the outflow of lava below was markedly diminished. On the 21st the activity at the crater was distinctly at the first degree, but on the 22nd the second degree was attained. On that evening, however, a gush of lava showed the removal of the lateral obstruction to its outflow, and the central activity so diminished that the following night no reflection was visible. On March 29 and 31 the crater again showed the first degree of activity.

This was followed during the first week of April by a fresh outflow of lava, which still more increased during the next week. During the first four days of the month feeble reflection was from time to time visible from the crater. On the 12th, black dusty smoke was puffed out from time to time. On visiting the Atrio, I

found the lava that flowed had formed the mound above spoken of, surmounted by the fumaroles figured.

During the night of May 3–4 fresh portions of the crater wall collapsed and blocked the vent, so that during the following day hardly any vapour crowned the summit of the volcano. By the next day the increased tension of the vapour was sufficient for it to force its way through the obstruction, and much black sandy smoke escaped during the 5th, 6th, 7th, and 8th. Obstructing masses that had detached themselves from the crater sides again plugged the vent on the 27th and 28th, followed the next day by dark, sandy, and dusty smoke.

The flowing lava showed few new phenomena, with the exception of the fine examples of conical and tubular spiracles formed above the lava exit at the same locality, but above one set figured in the last report that are now buried. One of these is unique on account of its curved overhanging form. It has been ejected at the highest point of the new lava, and quite at the foot of the great Vesuvian cone. I can only explain its inclination at the lower part, other than that the escape of vapour and lava fragment were projected upwards and outwards in a plane radial to the volcanic chimney which corresponds with the orientation of the fumarole. This lateral projection seems to have gone on for some time, so that many of the blobs of lava blown out, fell and formed a support for the inclined tube. As the blasts escaped more feebly the edges of the mouth became more solid, and so the lower lip diverted the column more in an upright direction, until the growth became almost vertical. The whole effect is to produce a large mass somewhat resembling a recumbent animal with its neck and head erect.

About twenty yards more distant from the foot of the cone was another large, obtuse, conical-shaped fumarole, which had been broken away on one side and well exhibited the dome-like interior covered by stalactitic lava. These constitute very fine examples of what kind of spiracles may be built up on the surface of a coarsely crystalline lava, such as that now issuing from Vesuvius. They differ very considerably from those described and illustrated by Dana and others from Hawaii, as also those formed on acid lavas at Reunion, of which one or two figures have been published.

At the time of visiting these fumaroles I was accompanied by my friend Dr. R. D. Roberts, of London, who was much interested in these striking formations. He is a man of average height, so that the dimensions of these fumaroles can be judged of by comparison with his figure by their side (photographs exhibited).

At the summit of the great cone few changes have occurred beyond the further enlargement of the crater. When slips took place from the edges, dark dust-laden vapour was puffed out from time to time. On one or two occasions the lava rose sufficiently high in the chimney, combined with the sufficiently strong explosions, to project a few lava cakes beyond the crater edges. The bottom of the crater has been invisible from the large amount of vapour present on each occasion that I visited the mountain summit. The extreme trunca-

tion of the old eruptive cone is very strikingly seen by comparing the photograph, taken a little over a year ago and published in the last report, and the present one (photographs exhibited).

Since I quitted Naples an actual crateret has opened in the Atrio at the point where these fumaroles stood, and several gushes of lava have taken place. I shall more fully report on these new phases on my return to Naples.

III.—Ninth Report of the Committee, consisting of Professor T. Wiltshire (Chairman), Dr. H. Woodward, and Professor T. Rupert Jones (Secretary), on the Fossil Phyllopoda of the Palæozoic Rocks. (Drawn up by Professor T. Rupert Jones.)[1]

EIGHT reports by this Committee have been handed in and printed, the last in 1890. Part I. of the 'Monograph on the British Fossil Phyllopoda,' by Prof. T. Rupert Jones and Dr. H. Woodward, published by the Palæontographical Society, contained twelve plates, illustrating thirty-nine species, belonging to four genera of *Phyllocarida* (*Ceratiocaridæ*) therein described. Part II. of that Monograph is now finished, and has five plates of twenty-eight species in seven genera (including some of both the bivalve and the univalve *Phyllocarida*).

The genera here treated of are *Hymenocaris*, *Lingulocaris*, *Saccocaris*, *Caryocaris*, *Aptychopsis*, *Peltocaris*, *Pinnocaris*, and *Discinocaris*.

I. Of *Hymenocaris* we know of only two species, both British, namely, (1) *H. vermicauda*, Salter, very common in some beds of the Lingula-flags in North Wales; and (2) *H. lata*, Salter, represented by a unique and distorted specimen from the same strata.

II. Of *Lingulocaris* there are (1) *L. lingulæcomes*, Salter; (2) *L. siliquiformis*, Jones; and (3) *L. Salteriana*, T. R. J. and H. W. These are from the Cambrian of North Wales.

III. *Saccocaris major*, Salter, from the Lingula-flags, and *S. minor*, T. R. J. and H. W., from the Arenig series, are described and figured.

IV. *Caryocaris Wrightii*, Salter, and the thinner *C. Marrii*, Hicks, from the Skiddaw slates of Westmoreland, are fully treated of; and it is suggested that the latter form is possibly due to a sexual difference.

V. Of *Aptychopsis* we have recognized thirteen species, mostly British: 1. *A. prima*, Barrande, with its varieties *longa* and *secunda*, all Bohemian. 2. *A. Barrandeana*, sp. nov., and its variety *brevior*, nov. 3. *A. cordiformis*, sp. nov. 4. *A. lata*, sp. nov. 5. *A. glabra*, H. W. 6. *A. Wilsoni*, H. W. 7. *A. Salteri*, H. W. 8. *A. Lapworthi*, H. W. 9. *A. ovata*, sp. nov. 10. *A. subquadrata*, sp. nov. 11. *A. angulata*, Baily. 12. *A. oblata*, sp. nov.

Nos. 5 and 8 are probably represented among the several figures of various forms of '*Aptychopsis prima*' given by Barrande in his Syst. Silur. Bohême, vol i., Supplement, 1872, plate xxxiii.

Nos. 10 and 11 are from the Silurian of Ireland; the others (excepting No. 7, from South Wales) are from the Moffat series of South Scotland.

[1] Read before (Section C) British Association, Edinburgh, August, 1892.

VI. Three species of *Peltocaris* are (1) *P. aptychoides*, Salter; (2) *P. anatina*, Salter; (3) *P. patula*, sp. nov.

Like *Aptychopsis*, *Peltocaris* is an Upper-Silurian British form, with some representatives in the Middle Silurian.

VII. *Pinnocaris Lapworthi*, Etheridge, jun., a rare Silurian form, is known in Ayrshire and Westmoreland.

VIII. Of the round subconical tests, undivided except by the triangular nuchal notch, *Discinocaris* gives four species, all from the Upper or Middle Silurian of South Scotland and Westmoreland: 1. *D. Browniana*, H. W. 2. *D. ovalis*, sp. nov. 3. *D. undulata*, sp. nov. 4. *D. gigas*, H. W.

The British specimens here referred to belong to—(1) the British Museum; (2) the Museum of Practical Geology and Geological Survey of Great Britain; (3) Museum of the Geological Survey of Scotland; (4) Museum of the Geological Survey of Ireland; (5) Woodwardian Museum, Cambridge; (6) Museum of the Owens College, Manchester. The authorities of these institutions have courteously given us facilities (by loan or otherwise) for studying the specimens. For the loan of a large series we owe thanks to Mr. J. D. Brown, of Moffat, and for others to Dr. C. Lapworth, of Birmingham. The late Mr. James Dairon, of Glasgow, also obligingly lent us some specimens.

The *Pinacaridæ* (*Dithyrocaris*, etc.) have next to be described and figured in detail; and further descriptions and figures are required of the *Ceratiocaridæ*, for which the Committee have accumulated much material. Mr. J. G. Williams, F.G.S., of Ffestiniog, has lent the Committee a large series of North-Welsh Phyllocarids, including *Hymenocaris* and other genera, which will require careful study.

The chief additions since 1889 to published information about the Palæozoic *Phyllopoda* are—

1. Some notes on the Devonian *Estheria membranacea*, with a figure and description of an oblong variety, showing the concentric riblets and interstitial ornament, in the Geological Magazine, 1890, Pl. XII. Fig. 9, and 1891, p. 50.

2. In his Mémoire sur la Faune du Grès Armoricain (Annales Soc. Géol. du Nord, vol. xix. 1891), Dr. C. Barrois, after noting (pp. 147 and 149) that the little fossil, quoted by MM. de Tromelin and Lebesconte as *Cytheropsis subtestis* (Report Assoc. Franç. Congrès de Nantes, 1875 [1876], p. 23) is a *Primitia*, near *P. debilis*, Barrande, proceeds to treat of *Myocaris lutraria* at pp. 220 and 221, pl. v. fig. 4; and describes a small caudal spine, probably of a *Ceratiocaris*, p. 221, pl. v. fig. 3; also an abdominal segment and caudal appendages (style and stylets) of a new Ceratiocarid, namely, '*Trigonocarys*' [*Trigonocaris*] *Lebescontei*, nov. gen. et sp., pp. 222–226, pl. v. figs. 5 and 6; all from Guichen.

3. Description of Fossils from the Palæozoic Rocks of Ohio, by R. P. Whitfield (Annals New York Acad. Science, vol. v. December, 1890):—

Page 562. Phyllopoda, p. 563, *Echinocaris*, Whitfield (Amer. Journ. Sci. 1880).

At pp. 564–5, seven groups of *Ceratiocaridæ* are defined, according to their segments, etc.

Page 565. *Echinocaris sublevis*. Whitfield, 1880, pl. xii. figs. 12–14.

Page 567. *E. pustulosa*, Whitfield, 1880, pl. xii. fig. 13.

Page 568. *E. multinodosa*, Whitfield, 1880, pl. xii. fig. 16.

From the Erie shales of Ohio (=Portage and Chemung groups of New York State). See Amer. Journ. Science, third series, vol. xix. p. 36, etc., 1880.

At p. 572. *Aristozoe canadensis*, Whitfield, 1880, pl. xii. figs. 17 and 18, from the Trenton formation in the Ottawa basin of Canada. Locality unknown. Introduced for comparison.

See also Report British Association for 1885 [1886], p. 35.

4. The illustrated description of *Discinocaris Dusliana*, by Prof. O. Novák, in the Geological Magazine, 1892, p. 148. This is one of three specimens found by Herr Dusl in the strata of the "Colonie Haidinger," Bohemia, and referred to by Mr. J. E. Marr, F.G.S., in the Quart. Journ. Geol. Soc., vol. xxxvi. 1880, p. 617.

5. The fauna of the Lower Cambrian, or Olenellus Zone, by Charles D. Walcott (Tenth Annual Report of the U.S. Geological Survey, 1891 ?); *Protocaris Marshi*, Walcott, p. 629, pl. lxxxi. fig. 6 (Bull. U.S. Geol. Surv., No. 30, 1886, p. 148, pl. xv. fig. 1) :— an obscure subquadrate test (?), with a many-segmented body and a furcate caudal appendage (see our Seventh Report for 1889 [1890], p. 64).

IV.—On some *Dicynodont* and other Reptilian Remains from the Elgin Sandstone. By E. T. Newton, F.G.S., F.Z.S.

At the Aberdeen meeting of the British Association, in 1885, Dr. Traquair called attention to the skull of a *Dicynodont* which had been discovered in the Elgin Sandstone of Cutties' Hillock (=New Spynie). Since that time several other specimens have been obtained from the same place, some of which are the property of the Elgin Museum, while others belong to the Geological Survey of the United Kingdom. These specimens are now being worked out by the author, and this communication is a preliminary note on the interesting results which have been obtained.

All the reptile remains obtained from Cutties' Hillock are in the condition of hollow casts, the bones themselves having been dissolved away; this, it will be remembered, was the case with some of the examples of *Stagonolepis* from the Elgin Sandstone, described by Prof. Huxley, and the method of taking casts from the hollow cavities, which was adopted in that case, has been found of great advantage in the present instance. The blocks when brought from the quarry were more or less split open, exposing portions of the specimens. In some cases these cavities were traced out and developed with the chisel, while in others they were farther split open, thus allowing casts to be taken. In many cases these casts had to be made in several parts and afterwards fitted together. The time and labour involved in this task have been repaid by the

restoration of the skulls and parts of skeletons of several *Dicynodonts*, and one or two other equally remarkable forms of reptiles.

In most of these specimens, including that noticed by Dr. Traquair, the skulls are similar in form, although differing in minor details, and have a general resemblance to the South African *Dicynodon* and *Oudenodon*, some of them having small tusks in the maxiliary bones. With most of these skulls parts of the skeleton have been found. Two or three show the position of the vertebral column and ribs, but up to the present no definite centra have been traced; besides this there is evidence of scapula, clavicle, humerus, radius, and ulna, the humerus having the characteristic anomodont expansion of the two extremities. In two specimens the ilia are preserved. These forms appear to be distinct from *Dicynodon*, and probably represent at least two or three species.

Another skull presents most of the characters of *Ptychognathus*, but has a short muzzle and no teeth. The last, and by far the most remarkable skull of this series, is about six inches in length, and has the outer surface completely covered in by bony plates, the nostrils, eyes, and pineal fossa being the only aperatures. The chief feature of this skull is the extreme development of horns upon the face and cheeks, there being about thirty of these formidable defences varying from a fourth of an inch to nearly three inches in length, besides some smaller bosses. The dentition is pleurodont, and resembles very closely that of the living *Iguana*; the palate is lacertilian. but with the pterygoids united in front of the pterygoid vacuity. This skull reminds one very strongly of the living *Moloch* and *Phrynosoma*, but it probably finds its nearest ally in the *Pareiasaurus* from the south African Karoo Bed. The detailed description of these specimens is nearly completed, and will, it is hoped, be shortly published.

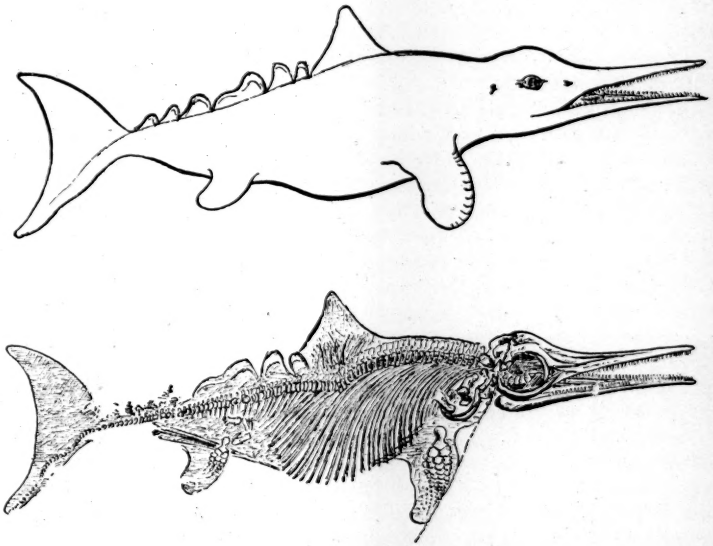
REVIEWS.

I.—The Paddles and Fins of Ichthyosaurus.

Ueber einen neuen Fund von *Ichthyosaurus* in Würtemberg. By Dr. Everhard Fraas. Neues Jahrb. 1892, vol. ii. pp. 87–90.

TRACES of the skin have long been known to occur with the remains of the skeleton of *Ichthyosaurus* in the fine-grained limestones and indurated shales of the Lias; and the descriptions of the integument of the paddle by Sir Richard Owen, Dr. Everhard Fraas, and Mr. Lydekker, are now familiar to most students. Hitherto, however, well-preserved specimens showing the precise contour of the animal, have been a desideratum; and we therefore note with especial pleasure the recent discovery by Dr. Everhard Fraas of an *Ichthyosaurus* with the nearly complete integument in the Upper Lias of Würtemberg. Through the courtesy of Dr. Fraas we are enabled to reproduce his drawing of the fossil, accompanied by an outline-restoration based upon the facts it makes known.

Zoologists commonly regard the Ichthyosaurian modification among reptiles as equivalent to that of the cetacea among mammals; and it is very interesting to find how remarkably dolphin-like is the restoration of *Ichthyosaurus* given by Dr. Fraas. There is a triangular dorsal fin, and also a large caudal fin; while a row of horny excrescences extends along the back from the former to the latter. *Ichthyosaurus*, however, differs essentially from the dolphins in having the great caudal fin vertically, instead of horizontally, extended. The vertebral column is continued to the point of one lobe, as in a heterocercal fish; but Dr. Fraas supposes that this was not the upper lobe (as in fishes), appearances suggesting rather that it was the lower lobe. In proportion to the size of the animal this fin is very large, and Dr. Fraas' discovery is an interesting confirma-



Skeleton of *Ichthyosaurus* exhibiting the contour of the integument, with an outline-restoration of the same.

tion of the surmise of Sir Richard Owen, who, many years ago, noticed the frequent dislocation of the tail of *Ichthyosaurus* at a fixed point, and could only explain the circumstance as due to the weight of a caudal fin dragging upon this part of the body after death.

As yet the specimen described by Dr. Fraas is unique, and some points in his interpretation of the fossil may perhaps admit of more than one view. We can only hope that further evidence will soon be forthcoming, and meanwhile express our appreciation of the enthusiasm and success with which Dr. Fraas is prosecuting his researches among the Fossil Reptiles of Southern Germany.

II.—Untersuchungen ueber die Organisation und systematische Stellung der Receptaculitiden. Von Hermann Rauff. Abhandl. der k. bayer. Akademie der Wiss. II. cl., XVII. Bd., III. Abth. pp. 647–722, Pls. I.–VII.

Researches into the Organization and Systematic Position of the Receptaculitidæ. By Hermann Rauff.

As part of a work on the fossil Sponges of Germany, Dr. Rauff has critically examined the Palæozoic fossils included in the family of the Receptaculitidæ, and in this Memoir he has given a more detailed description, with fuller illustrations of their structure, than has previously been attempted. The main characters of these fossils have long been known through the works of Billings, Gümbel, and other writers, but nevertheless some new and interesting points, well deserving of consideration, are here brought forward.

It is well known to palæontologists that the nature and relationship of *Receptaculites* and its allied genera have been fruitful subjects of discussion, and that they have at different times been placed in various divisions of the plant and animal kingdoms. In 1884, Hinde supported the view that they were sponges allied to the siliceous hexactinellidæ, on the ground that the individual elements or spicules—Meromen, as they are styled by Dr. Rauff—of which their walls are composed, very strongly resembled in general features, the spicules of recent and fossil hexactinellid sponges, and that, like these latter, they must have originally been composed of silica. In their present condition many of these fossils are of granular crystalline calcite; some, perhaps the majority of the specimens known, are only casts, more or less replaced by iron peroxide or other substances, others are now siliceous, but the silica in these is partially crystalline, and therefore not original, and occasionally a rare specimen occurs which is found to be composed of a finely fibrous carbonate of lime. In this latter condition there are traces of concentric layers of growth, and of an internal tube or canal in the vertical rays or pillars of the spicules, and this fibrous structure was regarded by Gümbel as the original skeleton of the organism; a view upheld and defended by Rauff in this Memoir. If this view is correct, all analogy with siliceous hexactinellids at once falls to the ground. The fibrous calcite in these particular specimens was considered by Hinde to be merely a replacement after silica in the same way that granular calcite very often replaces the silica in other fossil sponges, but Dr. Rauff maintains that this is mineralogically impossible; that no traces of the original lines of growth could be preserved in spicules thus replaced; and consequently that *Receptaculites* is a calcareous organism.

The question of the original mineral nature of these fossils is by no means a simple one, and one would like to have clearer evidence whether the fibrous carbonate of lime could possibly have resulted from replacement, or whether it is primarily of organic origin, as Dr. Rauff, following Gümbel, asserts it to be. There can be no

doubt, that the original mineral structure of *Receptaculites* and its allies must have been extremely liable to change, for in by far the greater number of specimens, as already mentioned, it has either been entirely removed or replaced by granular calcite, silica or other substances, in very much the same way as we find the originally siliceous skeletons of fossil sponges; whilst in only two or three specimens is the skeleton composed of this peculiarly arranged fibrous calcite, believed to be its original structure. A very striking exemplification of the difficulty of satisfactorily determining whether this fibrous carbonate of lime is original, or secondary, is afforded by the inability of Dr. Rauff to make out whether the fibrous structure now occurring in the axial portion of the horizontal or tangential arms of the spicules of *Receptaculites*, represents an originally solid portion of the arm, or is merely secondary infilling of a canal or cavity. These doubtful structures, spindles as they are termed, prove very durable, and remain when the walls bounding them have disappeared. Dr. Rauff is inclined to consider the spindles to have been originally solid, but if one might judge from silicified specimens they are probably secondary solid infillings of an original canal or cavity in the arms.

Dr. Rauff definitely accepts the calcareous nature of the *Receptaculitida* and concludes, in spite of the remarkable general resemblance of their spicules to those of siliceous *hexactinellids*, that they could have no relationship to sponges. At the same time he is unable to find in them any resemblance or relationship to any other group of fossil or recent organisms, and can therefore give no clue to their probable position, although he thinks that the question of their alliance to the calcareous Algæ, such as the *Siphoneæ verticillatæ*, put forward by Steinmann, Deecke, and others, deserves further consideration.

As regards the general structure of *Receptaculites* itself, Dr. Rauff adopts Billings' view that they were, when perfect, conical or subspherical in form like *Ischadites*, and consequently that the cup or platter-shaped examples, which are the only ones known up to the present, are incomplete. Theoretically there is much to favour this view, more particularly as in no specimen yet discovered are the margins of the open cups perfect, but on the other hand it is very peculiar that the supposed upper parts of the specimens should not have been met with ere now, considering that the basal cups are in some beds numerous and fairly well preserved. Similarly the author regards the cup-shaped forms from the Bohemian Silurian, placed by Hinde under *Acanthochonia* as merely incomplete specimens of *Ischadites*. A significant indication that open cup-shaped specimens of *Receptaculites*, rather than the assumed conical forms, were present on the sea-bottom is shown by the fact that in an example figured by the author (pl. iii. fig. 8) a Stromatopora has established itself on the inner or upper surface of the wall which must consequently have been open and uncovered at the time.

In well-preserved specimens of *Receptaculites Neptuni*, the inner ends or feet of the vertical rays or pillars are inflated, so that they

come in contact with each other, and form an inner or upper wall to the cup. In *R. occidentalis*, from Canada, this inner wall has been shown by Billings and Hinde to be regularly perforated by canals which communicated with the spaces between the pillars; but Dr. Rauff asserts, without having seen the Canadian specimens, that the perforations are only secondary, and not original; and that the inner wall is not penetrated by canals. But as far as can be judged from the author's figures of the inner wall of *R. orbis* (pl. iii. figs. 10, 10*a*; figs. 3, 4, p. 675), similar perforations are presented in it as in the Canadian species, and in this latter they are too distinct and regular to be explained away as produced by fossilization.

The author shows that the meridional arms of the spicules in *Receptaculites*, and possibly throughout the group, follow a very regular arrangement, whereby the arm directed to the nucleus or commencement of growth (proximal, Hinde; distal, Rauff) is tilted obliquely towards the outer surface, whilst the opposite arm points obliquely towards the interior of the organism.

Dr. Rauff considers that when complete the summit or upper end of *Ischadites* was completely closed, in spite of the fact that in all the specimens yet known in which the upper portion is preserved, there is a small aperture which communicates with the inner cavity. This apertural fact is attributed to subsequent injury in fossilization, or to artificial enlargement, by those who have studied and figured the specimens; but it may be noted that the existence of this summit aperture seems never to have been questioned previously. In another point the author differs from previous observers who have shown that the pillars or vertical rays in good specimens of *Ischadites Kœnigii* gradually taper to a point, and do not form an inner wall; but as a fragment of a poorly preserved drift-specimen shows trumpet-shaped inflations, the author concludes that these were the original terminations in this species, and explains the spicules with pointed ends as arising from subsequent alteration; but this explanation will certainly not account for the tapering spicular rays in some of the Gotland examples of *T. Kœnigii*, which show every indication of being complete.

Dr. Rauff refers to *Leptopoterion*, Ulrich, as a new genus of this family, characterized by the nearly uniform diminutive size of the summit plates of the spicules. The form has not yet been figured, and the only species has been described as a sponge.

In a promised future Supplement the author intends to describe the various species, and it is then to be hoped that he will be able to definitely settle some of the doubtful structural points, and at the same time bring forward some positive conclusions as to the systematic position of this peculiar family.

Some fresh information respecting the structure of *Sphærospongia tessellata*, Phill. sp., based on specimens from the Devonian rocks on the shores of Lake Manitoba, is given in a recently issued part of vol. i. Contributions to Canadian Palæontology, p. 259, pl. xxxiii. The summit in this species is stated to be formed by the convergence of the prolonged distal rays of the upper spicules in each longi-

tudinal row, and there are no indications that the hexagonal plates of the rest of the surface reached over the apical portion. If this determination is correct, there would be free communication with the exterior at the summit of the organism.

III.—Field Notes from the Shan Hills (Upper Burma). By Fritz Noetling, Ph.D. Record Geol. Surv. India, vol. xxiii. part ii. pp. 78, 79. Calcutta, May, 1890.

THIS records the interesting occurrence of Limestones curiously resembling the *Echinosphæriten-kalk* of the Baltic provinces and Sweden. The fossils collected are fragments of an *Orthoceras*, belonging to the group of the Regulares; Crinoid stems of two different types; and an *Echinosphæra* of very large size. The latter is said by the author to be a new species, and is named by him *E. Kingi*; since, however, no diagnosis, or even description, is given, we are unable to accept this species, and we trust that the learned Director of the Indian Survey will not in future permit his name to be thus taken in vain. If this *Echinosphæra* is really a new species, we fail to see how it bears out the author's contention that the red limestone of Upper Burmah is on "the exact horizon" of the Baltic *Echinosphæriten-kalk*, and that an arm of the sea in which that was deposited must have stretched right down to south-eastern Asia. The genus *Echinosphæra* is, for the rest, by no means confined to a single horizon in the Ordovician. F. A. B.

REPORTS AND PROCEEDINGS.

The Chapelhall "Shell-Bed," near Airdrie.[1] By Dugald Bell, F.G.S.

THE Chapelhall Shell-bed, near Airdrie, had often been adduced as proving a submergence of at least 526 feet. Mr. Bell reminded the members of what he had shown on a previous occasion—that the "shelly gravel" on Moel Tryfaen in Wales could not be accepted as a proof of submergence, there being every evidence that it is not really in place, or as laid down by the sea. There was a great difference between a submergence of 1360 feet and one of 526 feet, and we might more readily believe in the latter than in the former; but he thought the more it was considered the more doubtful and inconclusive did even the smaller submergence, as depending on this Chapelhall instance, appear. He described the locality, and pointed out how little was really known of the "deposit" in question. It was reported by Mr. James Russell, a mining engineer then living in the locality, as having been found while digging a well near the summit of one of the high ridges of Boulder-clay that abound in the neighbourhood. It was described by him as a bed of "reddish brick clay" containing marine shells, intercalated in the "tile" or Boulder-clay, which had a great thickness both above and below it.

[1] Report of a paper read before the Geological Society of Glasgow.

Mr. Smith of Jordanhill, who recorded the discovery in a paper read to the Geological Society in 1850, visited the locality, but does not appear to have seen the brick or shelly clay—only the "superincumbent matter which was left lying at the mouth of the well," and which he pronounced to be "true Till." He got some shells, however, from Mr. Russell, which seems to have been all of one species, *Tellina calcarea*. From that day to this, though the place had been visited by many geologists, yet, the well being built up, no one is known to have seen or examined this "shelly deposit." Mr. Russell stated that it was about two feet deep in the thickest part, but "thinned away rapidly on every side," so as to allow the Upper and Lower Till to come together. This had been confirmed by a new well having been sunk within a few yards of the old one without finding any trace of the "shell-bed." In short, it seemed to be quite a limited strip or patch of shelly clay, imbedded in the Till or Boulder-clay. And this appeared to be a very narrow foundation for all that had been built upon the section, namely, a period of severe Arctic conditions and massive land-ice; then a milder period of deep submergence; then a re-elevation of the land, and Arctic conditions and massive land-ice once more.

Mr. Bell commented on the many improbabilities involved in this intervening "deep submergence," whether regarded as due to an actual subsidence and re-elevation of the crust of the earth, or to the mass and attraction of the ice raising the general level of the sea in the Northern hemisphere. The latter was by far the more probable hypothesis, but neither by calculation nor by comparison with the facts presented in Norway and North America did it warrant us in assuming a submergence in this country of anything like 526 feet. There was absolutely no corroborative evidence for such a submergence, but much that led directly against it. No shells had been found at a similar level in other parts of the Midland Valley, nor in the numerous side valleys, where they would be more likely to be preserved than on this exposed knoll in the centre. None have been found in the Upper Boulder-clay, which, if all this valley had been a sea-bottom before the "second glaciation," should contain abundance of at least shelly fragments. Further, this shelly clay was said to have been deposited during a "mild inter-Glacial period," which would most probably accompany such a submergence; but the only species of shells found in it indicated not mild, but extremely cold conditions, this *Tellina* being a characteristic Arctic species not now found living in the British seas. In short, he thought the evidence was very strong that this limited and local shelly clay at Chapelhall, taking all we know of it, was not in any true sense a marine bed. The alternative suggestion was that "The layer containing these shells may have been transported (most likely in a frozen condition) by the ice-sheet, as in many other instances to which he referred. This appeared to be by far the most probable account of it; its position in the track of the old ice-sheet, and in front of an obstruction presented by the highest rising ground in the district, the nature of the organisms, and the

very colour of the clay (as reported) being different from the clays of the immediate neighbourhood; all pointed to this conclusion. He therefore urged that this Chapelhall clay should no longer be cited as a proof of submergence.

An interesting discussion followed.

CORRESPONDENCE.

THEORY FOR "CLEAT" IN COAL-SEAMS.

Sir,—It must surely be admitted by students of the Coal-measures in every country, where coal-mining is carried on to any extent, that coal-seams are the most persistent in extent of area, most uniform in composition and in homogenity of any strata of the series; and therfore may justly be considered the *typical* beds of it. The *master-joints* or "cleat" of coal are much more regular than those of any other strata; and I think Coal-beds were much more likely to shrink and crack *evenly* than the less persistent and ever-varying associated shales, clays, and sandstones. The least-disturbed Coal-areas exhibit the best-developed or most regular and typical "cleat." Thus, if "cleat" was formed or produced by shrinkage of the mass in cooling, due to elevation following deep subsidence (which I think is the generally accepted opinion as to what caused "cleat"); why, it may, and has often been asked, does the "cleat" (the direction of the main joints) usually run roughly N.N.W. and S.S.E., this being the general trend, not only in England but in the United States, and probably in many other countries? I have not come across any good reason in explanation of this fact, but reflecting on the point it occurred to me that possibly the following theory might account for it.

As the Coal-measures were upheaved or elevated at the end of the Coal period, the rocks would cool and consequently contract to some extent, and in contracting would crack, and thus the joints would be formed; but the cause of the joints taking lines roughly parallel with the earth's axis, or closely corresponding with *polarity* or *longitude*, I venture to think may have been due to the increased rotary velocity or greater centrifugal force acting upon the coal-seams as elevation proceeded—as they got further and further away from the earth's centre and so became more liable to open, split, and expand: in other words, *the tensional strength of the coal gave way along approximate N. and S. lines due to increased velocity of travel as a consequence of elevation and cooling.*

Mathematicians and physical scientists may possibly demonstrate my theory to be contrary to the laws of natural science. If they do, we must then look for some other explanation of the phenomenon of "cleat." At any rate it is hoped that this communication will be accepted or rejected on its merits or demerits, and that it will call up some criticism.

W. S. Gresley, F.G.S.

Erie, Pa., U.S.A., 11*th Aug.*, 1892.

A BED OF PEAT IN LONDON CLAY?

SIR,—I observe in the July Number of the "Journal of the Society of Industrial Chemistry" a notice of a short paper in which the use of the term "London Clay" is erroneous, or at least appears so to me. The writers mention and describe a bed of peat at the works of the New Thames Tunnel "underlying the London Clay." As the bed in question is only 12 feet below the surface this is scarcely possible unless there is some misprint. The peat is "composed chiefly of branches and trunks of trees, twigs, etc. It is about two feet thick."

It seems to me that the writers are referring to one of the exposures of the buried forest or peat bed so common around the southern coast of England. The error, if such it is, would be of slight importance were it not for the concluding sentence, "it has geological interest as showing that at a period *anterior to the formation of the London Clay* an abundant growth of trees and shrubs extended from some distance inland right down to the water's edge in this locality."

It seems from this that the writers have also mistaken some recent and local stratum for the "London Clay" of geology. Perhaps some one nearer to the spot than I am can correct the error if there is one. E. W. CLAYPOLE, D.Sc., B.A. (Lond.)

BUCHTEL COLL., AKRON, OHIO.

NOTE BY MR. F. C. J. SPURRELL, F.G.S.

SIR,—I have read Mr. Claypole's letter. His surmise that the bed of peat 12 feet below the surface at the spot described is part of the recent forest-beds of the South of England is correct. It is above and in no way connected with the London Clay. This term, applied to the alluvial blue clays of the Thames, was in use in Brunel's days, and I should think the author of the paper referred to had been reading up some old accounts of embanking, etc., of the last century, when the *blue* clay, wherever found, was supposed to be the same as the mass underlying London. F. C. J. S.

BELVEDERE, KENT, 24*th Sept.*, 1892.

THE PHOSPHATIC CHALK AT TAPLOW.

SIR,—Since, so far as I am aware, none but microscopic fish remains have been recorded from the Taplow phosphatic chalk, it may be of interest to mention that, on July 8th last, I found in the 8 ft. bed at Taplow Court Lodge, described by Mr. Strahan (Quart. Journ. Geol. Soc., 1891, p. 356), the detached crown of a shark's tooth $\frac{3}{8}$ inch long. Although this is insufficient for accurate determination, Mr. A. Smith Woodward informs me that it most nearly resembles the form of tooth described by Agassiz as *Odontaspis subulata*, but is rather large for that species.

BERNARD HOBSON.

GEOLOGICAL DEPARTMENT, OWEN'S COLLEGE, MANCHESTER.
September 13*th*, 1892.

ON THE GENUS *TREMATONOTUS*.

Sir,—During the preparation of my paper on *Trematonotus*, published in the Geological Magazine for August, I much regret having overlooked a valuable monograph by Professor Lindström, "On the Silurian Gastropoda and Pteropoda of Gotland" (*K. Svenska Vet.-Akad. Handl.* 1884), containing descriptions and figures of two new species of this genus, *viz.*: *T. longitudinalis* and *T. compressus*.

The former of these shells, to which my remarks will now apply, was stated by its author to resemble so closely *Bellerophon dilatatus* of James de Carle Sowerby that he would have identified it as such, but for the absence of any mention of dorsal perforations either in the original description or in McCoy's later account of the same species (Pal. Foss. 1852, p. 309).

It was reserved for the Reviewer of Professor Lindström's work (Dr. G. J. Hinde) to point out that "*T. longitudinalis* is identical with *Bellerophon dilatatus*; as the type specimen of this form shows distinct traces of the characteristic apertures on the dorsal keel" (Geol. Mag. 1885, p. 39).

An examination of the Sowerby type did not convince me that this evidence was complete; but Dr. Hinde has recently shown me another specimen of *B. dilatatus* in a better state of preservation, belonging to the Jermyn Street Museum (No. vii. $\frac{1}{3}\frac{1}{1}$) which possesses unmistakably the elongate perforations, so that all doubts, in my mind, are now removed as to the true nature of this classical species.

With regard to my *T. Britannicus*, although more ovate in contour and very deficient in its umbilical characters, through pressure, yet its ornamentation and perforations are so like the Swedish specimen that they may be looked upon as practically the same species. The effect of this will be that *T. Britannicus*, like *T. longitudinalis*, will fall into synonomy under the older name of *T. dilatatus*.

In conclusion, I wish to record my indebtedness to Dr. Hinde for assisting me in this determination, and to assure Prof. Lindström how sorry I am that his monograph escaped my attention.

Nat. Hist. Museum,
October 12*th*, 1892.

R. Bullen Newton.

ON THE FLEXIBILITY OF ROCKS.

Sir,—With regard to the localities in Durham at which Flexible Limestone occurs. I find that Prof. G. A. Lebour refers to several in his "Outlines of the Geology of Northumberland and Durham." At the time of writing the description of this rock published in the March Number of the Geol. Mag. (1892), I had no opportunity of referring to this work. Prof. Lebour kindly informs me that the variety from Marsden loses its flexibility after being kept in a dry place for some time. My specimens from Sunderland do not appear to have undergone any loss of flexibility as yet.

Royal College of Science,
October 3*rd*, 1892.

George W. Card, A.R.S.M.

THE CONISTON LIMESTONE SERIES.

Sir,—The Geologists Association visited the exposures of the volcanic rocks on the flanks of Roman Fell under my guidance, on the occasion of the Long Excursion to Cumberland in 1889, when they had an opportunity of examining the sections which it was the chief object of my paper on the Coniston Limestone series to describe. Other field geologists have at different times examined the same facts with me. The sections are not very easily found, and the relation of the various rocks to each other is certainly not very clear at first sight. But after repeated examinations of the Ordovician rocks of the whole of the area enclosed by the Pennine Faults it becomes evident that there are three well-marked horizons on which volcanic rocks occur. The highest of these is intimately associated with the base of the Coniston Limestone. The lowest consists of a series of tuffs of subaqueous origin, which are clearly interstratified with argillites resembling the Skiddaw Slates. These are the rocks of the Milburn Series, and are the submarine equivalents of the lower half of the Borrowdale volcanic series. The third includes the very peculiar set of volcanic rocks which I have described as the Helton Moor volcanic series. These are quite different in lithological character from either of the other two, and as they cannot be newer than the first, nor older than the second, they must be of age intermediate between the two. All three types occur side by side on Helton Moor, as I have pointed out already on several occasions. It is in association with these that the shales of Lycum Sike occur. We have the best possible authority for the occurrence in these beds of Lycum Sike of *Trematis corona*, which occur also in the shales belonging to the Coniston Limestone Series. *Trematis corona* is here, therefore, not available as a characteristic fossil, for the beds of Lycum Sike are separated by a considerable, if unknown, thickness of other rocks from the true Coniston Limestone Series [1] This latter overlies the rocks of Dufton and Knock Pikes, while the beds of Lycum Sike lie at an unknown distance below.

Much more is involved in this question than a mere error of delineation, which no one who has attempted to map the complicated area in question could well avoid, here or there.

Unless I am very greatly mistaken it is the middle and the upper group of these volcanic rocks which occur at the northern end of the exposure near Melmerby, and it is the same two groups which form the volcanic groups on the north-east side of the Lake District. And, furthermore, I strongly suspect that there is a considerable unconformity between the two higher, or Bala, volcanic groups, and the Arenig and older rocks upon which they lie in Cumberland.

I have little doubt that the Ordovician tuffs of the Craven area also correspond to those of Helton Moor and Dufton; but whether the Ingleton Green Slate series lies unconformably below these, or

[1] I regret very much that I did not write to Prof. Lapworth regarding what I understood him to say about the two horizons of *Trematis corona* at Girvan. I did not like to trouble an exceptionally busy man upon a matter that I thought he had already stated quite clearly. The error is mine.

whether it represents the materials of the Milburn series mingled with detrital matter from the seaward margin of the Borrowdale volcanoes will probably long remain a matter of opinion.

In conclusion I may state that I should be glad to conduct a party of field geologists over the areas here referred to if the excursion can be arranged for the summer months. If Mr. Marr should care to be of the party, so much the better.

Edinburgh Museum of Science and Art, J. G. Goodchild.
10*th October*, 1892.

SHAPES OF SAND GRAINS.

Sir,—It is interesting to find that my friend Mr. Reade admits the rounded sands in the Glacial deposits at Moel-y-Tryfaen "may be treated as erratics."

This view has been held by many glacialists of the anti-submergence school for years. In a paper read before the Liverpool Geological Society in December last, I stated that under the microscope the glacial sands found under the cliffs bounding the Mersey were almost undistinguishable from those on the shore.

But this fact gives no support to the belief that marine conditions obtained during the deposition of those sands. It does not follow that the sands have been rounded by marine action at all.

It is particularly unfortunate that Mr. Reade should have cited the sands "which he has been living on and working in as an engineer for the last twenty-five years" as examples of sea-worn grains.

Not only is the shore skirted by sand dunes whose bases are washed by the tide, but the grains themselves have most probably been derived from the Triassic and Permian rocks which form the solid geology of the district.

The remarkable roundness of grain which characterizes many beds in these formations is well known.

Not less striking than the roundness is the uniformity in size of the grains in some beds. Some agent has been at work which is capable of sifting.

Through the kindness of various friends I have received specimens of sands from many parts of the Desert of Sahara. In one case I had examples from different depths at the same place. The underlying grains are small in size, fairly angular, and contain a large proportion of ferruginous grains. The upper layer is composed of larger grains, extremely well rounded and very uniform in size. In wind-borne material we should expect a sifting due to the varying resistances offered to the wind by the sand particles.

"Desert sands," according to Mr. Reade, "are of course out of the question in glacial geology;" but in the present case it is possible that "desert sands" of a former period may not be "out of the question" and the roundness of grain may have little importance in Glacial Geology.

J. Lomas.

University College, Liverpool,
October 13*th*, 1892.

MISCELLANEOUS.

The Divining Rod Again.

Every now and again public attention is drawn to remarkable discoveries of water, obtained professedly through the medium of the Divining Rod. We have on former occasions referred to the use of this rod in the search for minerals as well as water, and attention was lately drawn to the subject in "Natural Science" for June, p. 253. More recently there was an announcement in the "Morning Post," of an astonishing discovery of water at Fishborne and Wootton, in the Isle of Wight, "by the successful use of the Divining Rod." Referring to this matter in the "Isle of Wight County Press" for September 24th, Mr. G. W. Colenutt remarks that previous attempts had been made to get water by sinking wells in the *impervious clays* of the Osborne Beds; the 'Diviner' went to the top of the hill where there is a capping of plateau gravel some twenty feet in thickness. This gravel forms a first-rate reservoir for the surface soakage, which is partly upheld by the Osborne and Bembridge clays, and partly thrown off in the form of springs. The 'Diviner' advised digging *in the land of springs* and—water was found! Mr. Colenutt observes that the usual dramatic incidents took place; but Geology cannot offer any explanation of the grand *finale* of the workmen "rushing out of the well in order to avoid the water."

A New Geological Map of Scotland, by Sir Archibald Geikie, D.Sc., LL.D., F.R.S., etc., has just been published by John Bartholomew & Co. The scale is ten miles to an inch, and the topography is based on the latest Ordnance Survey. The geology, embodying the latest work on the Geological Survey, summarizes our knowledge up to date; so that the map, together with the concise Descriptive Memoir that accompanies it, furnish us with an excellent index to Scottish geology. The colours are very clear, and they show many divisions in the old Highland Schists (termed Dalradian), as well as the Lewisian Gneiss, Serpentine, etc. Cambrian, Silurian, and succeeding formations are duly represented, as well as many varieties of Igneous rocks. A number of longitudinal sections illustrate the structure of the country. It should also be mentioned that parts of the N.E. of Ireland, and the adjoining tracts of Cumberland and Northumberland are likewise coloured geologically. The price of the map, folded in cloth case is only 6*s*.

Exploration in British East Africa.—Mr. John Walter Gregory, B.Sc., F.G.S., one of the Assistants in the Geological Department of the British Museum (Natural History), Cromwell Road, has obtained permission to accompany, as Naturalist, the expedition led by Lieutenant C. H. Villiers (Royal Horse Guards) to Lake Rudolph, East Africa. The route taken will be from Kismahu, at the mouth of the Juba River, to Barderah, thence to Lake Rudolph, the shores of which will be explored, and the party will return across Somali-land to Berbera, opposite Aden. The country is new, and valuable results are sure to reward so able a Naturalist as Mr. Gregory.